Confidence in Claims

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Contents

Preface

This work is about claims that experts make in various academic disciplines, and about how features of disciplines should affect our confidence in the correctness of those claims. Our field of study is work in the full range of disciplines, covering mathematics, the natural sciences, the social sciences and the humanities.

Disciplines differ from one another in several ways. Quantification and mathematical argument are the norm in some disciplines, but are rare in others. Some disciplines use experiments, while others rely on sources. And so on. But disciplines also have things in common. These include both the aspiration to get things right, and fundamental principles like respect for evidence and a requirement to argue rationally. We seek to lay out the differences and the commonalities in detail, and to assess the effects on our confidence. We also explore reasons why disciplines have their features.

The contents and the practice of disciplines are inseparable, so themes introduced in earlier chapters recur in later ones. But the broad plan is as follows. There is some scene-setting in chapters 1 and 2. Then in chapters 3 to 5, the main focus is on the claims that are made within disciplines. In chapters 6 to 9, it is on the practice of disciplines.

Debts to other authors are recorded in the footnotes. There is also a great debt to the staff of the British Library, the University of London Senate House Library and Cambridge University Library, to those who keep the world wide web running, and to those who create and maintain online repositories of academic papers. Finally, thanks are due to Anna Hughes for combing the text to find unsound arguments and typographical errors. The author is entirely responsible for the faults that remain.

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Work in epistemology is not normally treated with the caution that is appropriate when work has commercial significance, but for the avoidance of doubt, the author declares that he has not received any funding and has no competing interests.

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References

References have been given in the form of chapter and section numbers when they identify passages precisely enough. This approach should be increasingly useful with the rise of the electronic text. Roman numerals have been converted to Arabic numerals when they merely give volume, chapter or section numbers, rather than being parts of titles.

References to Plato include Stephanus numbers, and references to Kant's *Critique of Pure Reason* include the usual A and B page numbers.

Some of the books to which we refer exist in several editions, and material may appear, disappear or move around from one edition to the next. If a reference does not appear to point to the right place, the first step is to check the bibliography to see which edition to use.

Cross-references within the text are given by section number. A reference to a general line of argument is to everything that falls within the section. For example, a reference to section 3.4.2 is a reference not only to what comes immediately under the heading so numbered, but also to what comes under the headings numbered 3.4.2.1, 3.4.2.2 and so on. But when a reference is to some specific

point, the point will be found in the material that comes immediately under the heading. Thus if reference were made to a specific point in the form "section 3.4.2", the point would be found before the heading numbered 3.4.2.1.

The PDF file of this work at http://www.rbphilo.com/ is searchable, so there is no index. However, readers should be aware that some software will not find a phrase in a PDF file when the phrase runs over two lines, or a word when it is hyphenated over two lines.

Summary

Chapter 1: Disciplines and Claims

1.1: Our main question

Our main question asks how various features of disciplines should influence our confidence in the correctness of claims that receive the assent of expert researchers, and that thereby come to be accepted. (We shall say “confidence in claims”, or simply “confidence”, and “researchers”, for short.)

We need to take it that there is a legitimate notion of correctness. Researchers aim to make correct claims.

We shall use a fairly liberal notion of expert researchers.

Claims will count as accepted when they enjoy the assent of the generality of researchers. Unanimity is not required. A formal process of acceptance is also not required.

We are interested in legitimate influences on our confidence in claims in general, rather than on our confidence in individual claims.

We recognize the existence of positive support for claims.

The notion of features of disciplines has a wide scope.

1.2: The corpus

Each discipline has a corpus, the claims to which the great majority of researchers assent.

1.3: Norms

The norms of disciplines set out how research should be conducted. There are several types of norm.

1.4: Differences

Disciplines differ in several respects. The availability of deductive relationships between propositions, and the extent to which pervasive claims constrain the making of other claims, will both be significant.

1.5: A scale of disciplines

We can arrange disciplines on a scale, ranging from mathematics and the natural sciences at the bottom end, through the social sciences, up to the humanities.

Chapter 2: A Framework

2.1: Disciplines, topics, accounts

We shall identify disciplines, topics within those disciplines, and accounts of those topics.

Sub-disciplines, however small, will be regarded as disciplines.

Disciplines all have their own norms. Norms can have a significant favourable effect on our confidence.

Norms tend to be defined most precisely in mathematics and the natural sciences, less precisely in the social sciences, and least precisely in the humanities.

An account is a set of claims, unified by their concern with some single topic and by relationships to one another which structure the account. Accounts of topics may be comprehensive or fragmentary. And they may be more or less closely tied to specific occasions.

There may be scope to regard a single account as comprehensive. Alternatively, and particularly high up the scale of disciplines, there may need to be several complementary accounts because no one account could be regarded as comprehensive. And in any discipline there may be accounts that conflict with one another.

2.2: Idioms

Disciplines may be conducted in the physical idiom or the human idiom.

The physical idiom is the only idiom that is used in most of the natural sciences. It conveys information in terms that are independent of specifically human ways of conceiving the world.

Accounts given in the physical idiom would be intelligible to a wide range of rational beings.

The human idiom is used in the social sciences and the humanities.

The human idiom is the idiom of the intentional stance. It is used to express the contents of psycho-social understandings. It is also used when putting such understandings to work in explaining human conduct.

A psycho-social understanding comprises concepts and principles that allow people to explain human conduct by picking out such things as characteristics, habits of thought, drives, motives and intentions. The human idiom has the vocabulary for such things.

It is unlikely that researchers who used the human idiom could renounce our everyday vocabulary and make use of a purely technical idiom. And even if they could do so, that might not be a good move.

Accounts given in the human idiom would only be intelligible to a narrow range of rational beings.

Use of the human idiom might introduce bias into accounts, but it should be a harmless bias.

A look at historical accounts of human conduct brings out special considerations that support the use of everyday psycho-social understandings or modified versions of them.

2.3: Ways to order a scale

Scales of disciplines could be constructed by reference to various features of disciplines.

The use of different features would produce scales that were similar, but the similarity would not prove anything.

Scales would be messy, with partial orderings and some overlaps of disciplines, but that will not affect our argument.

Disciplines stand in relationships of dependence on one another, but we shall not use those relationships to construct a scale.

Chapter 3: Relationships between Propositions

3.1: Introduction

Relationships between propositions play important roles in the processes of making and assenting to claims. Propositions may stand in deductive or non-deductive relationships to one another. Arguments for claims may be built up in reliance on several relationships between propositions, or they may rely on single relationships.

3.2: Deductive relationships

Deductive relationships can allow claims to be supported, either by deducing them from other claims or by deducing consequences of them that are found to be acceptable.

There may be choices of rules of deduction.

The corpus of a discipline often supplies extra premises that are needed in order to make relationships deductive.

Deductive relationships may be presented with varying degrees of formality.

3.3: Non-deductive relationships

Non-deductive relationships may be almost as strong as deductive relationships, or they may be weaker.

The corpus of a discipline can supply background that makes it possible to argue for the existence of non-deductive relationships.

The use of non-deductive relationships to conduct arguments is essential in many disciplines, and it can be perfectly well-controlled.

3.4: Availability

The availability of deductive relationships varies as between disciplines.

One reason why deductive relationships are less widely available in higher disciplines is that the objects of study are complicated.

Another reason is that in higher disciplines, concepts are often given their content using methods that do not make content fully determinate.

Concepts may have vague extensions. Vagueness is more common higher up the scale of disciplines.

Vagueness of extension can limit the availability of deductive and near-deductive relationships, their usefulness in testing, and the extent to which they can be used to reach conclusions about objects of study.

3.5: Relationships and assent

Both deductive and non-deductive relationships can be used to appraise claims.

Our confidence in claims may be influenced both by the strength of relationships, and by the presentation of arguments that support the existence of relationships from premises to conclusions.

Chapter 4: Constraints on Claims

4.1: Pervasive claims

Pervasive claims may constrain the making of other claims. This can have favourable effects on our confidence.

There are indicators that claims are pervasive, but there are no definitive criteria.

Pervasive claims can be justified at least partly by their fertility.

Determinacy in the content of concepts is important to the favourable effect of the use of pervasive claims on our confidence.

4.2: Mathematics

In mathematics, large-scale deductive structures and highly influential pervasive claims are the norm. There are favourable effects on our confidence.

Researchers who use mathematics to understand the world choose the parts of mathematics to use. This stage of choice both helps to explain the enormous success of mathematics as a tool to understand the world, and saves mathematics itself from being beholden to the state of the world.

4.3: The natural sciences

The natural sciences must represent the world.

The world is very complex, and this has significant consequences. It is impossible in practice to derive the contents of most natural sciences from fundamental physics, there are disciplines in which exceptionless laws are not widely available, and in some disciplines concepts that suffer from indeterminacy of content and vagueness of extension must be used. The scope for pervasive claims to have favourable effects on our confidence is therefore limited.

4.4: The social sciences and the humanities

The severity of constraints on the making of claims is even less in the social sciences and the humanities. We must seek other sources of confidence.

There is scope to develop theoretical versions of some disciplines, within which deductive relationships may be readily available and pervasive claims may impose severe constraints. But researchers must be cautious about applying the results of such work to the world.

Researchers may use frameworks, whether chronological or topic-based, to help them organize their disciplines. Such frameworks can be very helpful. There are risks in their use, but those risks are not great.

4.5: Types of quantification

Quantification may be strong, linked to the formulation of unified theories, or weak, merely recording patterns.

Chapter 5: Explanation

5.1: Explanations and claims

Explanations are of primary importance in all disciplines. Phenomena that are explained may take a wide variety of forms.

Explanantia will comprise general claims, such as laws and principles, and claims of particular fact. It is important to distinguish these two types of claim. Explananda may be treated as single claims.

When researchers endorse an explanation, they will assent both to its claim to explain (the claim that it is an explanation), and to claims that play explanatory roles.

Claims that play explanatory roles only gain support from playing those roles if the explanations in which they do so are better than explanations in which they do not do so.

5.2: Explanation using laws

In some explanations, the general claims in the explanantia are all laws. We shall use a fairly generous concept of a law. Laws may be exceptionless laws, laws with exceptions or statistical laws.

When laws are exceptionless, explanations are likely to forge strong connections from explanantia to explananda. It may be possible to show that the correctness of claims in an explanans is sufficient, and perhaps also necessary, for the correctness of the claim in the explanandum. And explanations may very well be contrastive. There are also likely to be good prospects for testing the laws and for embedding them in theories.

Where laws have exceptions, the connections from explanantia to explananda are likely to be weaker, and our confidence in the correctness both of claims to explain and of claims that play explanatory roles may be diminished. The degree to which explanations are contrastive may be reduced, and it may be harder to test the laws. The laws may sometimes need to be embedded in the corpus in a way that is looser than the embedding of laws in theories.

The presence of ceteris paribus clauses in laws may limit support for claims to explain and claims that play explanatory roles, although it will not always do so.

When several laws are combined in an explanans, there is an increased risk that unanticipated exceptions to the predicted combined effect will arise. That weakens the connection from explanans to explanandum.

Explanations that use statistical laws can forge reasonably strong connections from explanantia to explananda, so long as they are explanations of other statistical laws or of statistics for large sets of individuals.

Explanations that use statistical laws may stand at some distance from the mechanisms of the world. But they can be contrastive.

5.3: Causal explanation

We are here concerned with explanations that make use of general causal claims.

Causal explanations give a strong impression of getting to grips with the workings of the world.

Explanations may identify mechanisms. These may range from mechanisms that are described by identifying elements that push and pull one another or do something similar, up to mechanisms that are described by mapping causal influences in some detail.

It may be more or less possible to identify, or to gesture at, relationships between identified mechanisms and what fundamental physics says goes on in the world. This becomes harder, and eventually impossible, as we go up the scale of disciplines.

Even if the values of variables are related, there may be no mechanism to be found. Even some sophisticated analyses may not distinguish causation from other associations of values.

The use of general causal claims in explanations can increase our confidence in claims to explain.

The identification of mechanisms can increase our confidence both in claims to explain, and in claims that play explanatory roles. It is particularly helpful to identify machine mechanisms, and to identify mechanisms which are such that it is possible at least to gesture at relationships to what fundamental physics says goes on in the world.

5.4: Explanation using principles

Principles are general claims that do not qualify as laws. They give guidance as to what is to be expected.

Explanations that use principles are not like explanations that only use laws. They render explananda unsurprising and comprehensible by showing how explanantia and explananda hold together.

They do this by using principles that are drawn from a wide background, usually an understanding of human nature.

Even conduct that at first sight appears irrational can be explained, but additional work is then required.

In sociological and historical explanations, principles are likely to play dominant roles. But laws can still play essential roles by connecting events.

The holding together of an explanans and an explanandum needs to amount to more than their consistency. Measures of coherence can give some idea of how to give substance to the notion of what is required.

A narrative sets out what happened on a particular occasion and why it happened. We shall use a generous concept of a narrative.

A narrative can explain by fitting events together in a pattern. Both senses of the word "colligation" are relevant.

Narratives must be faithful to the facts, but this can be a challenging notion when events have been brought under organizing concepts.

The giving of explanations that use principles needs to be controlled. Controls may not be as tight as they can be when explanations only use laws, but some controls are available.

There are controls over the use of principles.

The creation of narrative explanations can be controlled by the need to have them endorsed by other researchers. A claim that a narrative is explanatory can be supported by showing how principles are put to work to link events. A narrative can be tested by attempting to add detail, by attempting to integrate the narrative with other narratives at different levels of generality, and by using methods of analysis that are drawn from fields other than the primary field of the narrative.

We may have some concerns about explanations that use principles. Researchers might be trapped in a mistaken mindset. When several complementary explanations are available, there may be no claim to explain that enjoys the assent of the generality of researchers. And when there are conflicting explanations, it may be hard to have much confidence in claims that enjoy the assent even of the majority of researchers. We may also be concerned at the lack of a systematic way to resolve conflicts. And there may be scope to redescribe explananda in order to facilitate the giving of explanations, scope that could be misused.

Explanations that use principles can have some contrastive power, although the fact that phenomena are identified and

principles are then selected means we must be cautious about attributions of contrastive power.

Narratives may include causal claims that are one-off rather than general. Such causal claims can be legitimate, because of the supervenience of large-scale phenomena on the microphysical. And while such claims may well be open to challenge, the making of them is not an undisciplined activity. But it can be inappropriate to explain the actions of specified individuals by concentrating exclusively on external causes.

5.5: Different disciplines

Laws are less readily available in higher disciplines than in lower disciplines. There are some laws in the social sciences, but their coverage of fields of study is patchy. Explanations that only use laws are therefore more common low down the scale of disciplines.

In disciplines that are concerned with human conduct, it can be inappropriate to give explanations that only use laws. Such explanations would not capture our sense of free decision and action. If explanations use principles, that sense can be captured.

The availability of laws can be increased by making simplifying assumptions, as for example is done in economics. Acknowledgement of the complexity of the world can reduce the availability of laws again. But this effect can sometimes be avoided, depending on the interests of researchers.

5.6: Erklären and Verstehen

Erklären typically involves showing how the regularities and the mechanisms of the world give rise to some observed phenomenon. Verstehen requires a reader to see people

whose conduct is to be explained as possessing human points of view, that is, as investing people and events with meaning and value, as responsive to those investments, and as having a sense of being self-directed agents.

One mark of Erklären is that an explanation is given in terms which make irrelevant the human natures of those who give the explanation and those who grasp it. But the achievement of Verstehen relies on the human nature of the reader.

The achievement of the type of Verstehen that interests us confers access to propositional information which can be shared with other human beings. Verstehen in our sense is not merely a feeling of empathy. And there are other forms of understanding which should be distinguished from Verstehen.

The distinction between Erklären and Verstehen is related to the distinction between explanations that only use laws and explanations that use principles. Possession of a human point of view is required not only to achieve Verstehen, but also to make evident the explanatory force of many explanations that use principles, while it is only rarely required to make evident the explanatory force of explanations that only use laws.

5.7: Special types of explanation

Explanations that use case studies allow close attention to detail. But caution is needed when researchers generalize from case studies or theorize on the strength of them. The cases studied may not be representative of the cases that exist, and they may not adequately represent variation between those cases. In addition, the interpretation of cases needs to be controlled. Fortunately, there are norms and special methods which can lessen such concerns.

Explanations that make use of results are unproblematic when the actions of intelligent agents to achieve results are studied, although there are issues regarding unintended consequences. Such explanations are much more problematic when unconscious nature is studied.

Some explanations use mathematical models. We may be concerned that these models may not accurately represent the world. It is important to take care when interpreting the results of work with models, and not to assume without argument that results about the world have been established. There are however ways in which the necessary further argument can sometimes be supplied.

Issues about relationships with the world also arise in connection with simulations. In addition, a special issue arises in connection with simulations when the individual steps in them cannot be understood as rational steps.

Chapter 6: Norms and Concepts

6.1: Norms

Norms may govern both the conduct of research and the appraisal of claims. Norms that govern the appraisal of claims also influence the conduct of research, because researchers want to see their claims receive the assent of other researchers.

The norms of mathematics are special, and are particularly well-defined and strict.

In other disciplines low down the scale, in which deductive relationships between propositions and relationships that are almost as strong are plentiful, norms range from the very general to the very specific.

There is a norm that researchers should not make or assent to claims unless the claims have adequate positive support. The notion of positive support is perfectly legitimate. The views of Pierre Duhem and Willard Van Orman Quine may give us pause in relation to supposedly crucial experiments, but evidence can still lend very strong support to claims. Observance of the norms of the natural sciences can increase our confidence.

Disciplines higher up the scale, in which there are fewer deductive relationships between propositions or relationships that are almost as strong, also have their norms. But while norms that relate directly to the collection and the analysis of evidence may be generally agreed, there is less agreement on norms that govern arguments for general claims.

One useful norm in such disciplines is that explanations must comport with psycho-social understandings. Another useful norm is that tension between accepted claims should be avoided, or at least kept to a minimum.

6.2: Formative concepts

Formative concepts are the concepts that have a significant effect on how researchers approach the objects of study in a discipline.

Researchers sometimes have a choice as to whether to treat certain concepts as formative.

In the natural sciences, choice tends to be limited. This is connected with the fact that choices of different formative concepts will lead to the production of different accounts. There can be different accounts of a single topic in the natural sciences which conflict with one another, but that is undesirable. When it happens, it may be that no claim with competitors comes to be accepted.

In the social sciences and the humanities, there is more likely to be choice. There is wide scope to give a range of accounts of each topic, and there is more freedom than in the natural sciences to devise new concepts.

Choice in the social sciences and the humanities is however not uncontrolled. A set of formative concepts should make it possible to give accounts of a range of topics, such that those accounts would form a coherent set. And when a discipline is concerned with the conduct of individuals, a set of formative concepts must allow researchers to draw on a psycho-social understanding.

The availability of choices need not have a serious adverse effect on our confidence.

6.3: Pluralism

"Pluralism" is an umbrella term for a range of lines of thought. We concentrate on pluralism in the sense of a range of approaches within a given discipline, and on the advantages of pluralism.

A contest between approaches or between claims can be beneficial, because the challenge of answering the advocates of other approaches or claims is a route to progress.

A choice of approaches or claims can give rise to an ensemble of explanations or other accounts. Such an ensemble can confer greater understanding than a single account.

It is however not at all clear that disagreement is good in itself, and our interest in correctness debars us from supporting that idea.

Chapter 7: Experiments and Sources

7.1: The distinction

Experiments are set up to yield evidence of some desired type. Sources were not created in order to yield evidence, and they must be taken as they are. Data obtained from surveys resemble data from sources.

7.2: Experiments

Experiments are standardly used low down the scale of disciplines, and there are norms that govern their conduct.

Sometimes there are questions to ask about the extent to which experiments can allow claims to be made about the world outside the context of the experiments.

7.3: Sources

We distinguish the interpretation of individual sources from the statistical analysis of survey data.

The interpretation of individual sources can be a complex business. If sources were intended to convey information, the intentions of their creators must be discerned and the contexts within which the creators worked must be understood. If sources were not intended to convey information, they do not convey a manifest message that researchers can use as a starting point.

Sources are sometimes interpreted in certain ways in order to allow explanations to be given. Researchers may add plausible details that are not directly evidenced by sources, so as to allow the sources to support satisfactory narratives. Researchers may also interpret sources under the guidance of optional formative concepts.

There are several ways in which the use of sources by researchers can be scrutinized by others.

The use of surveys is for our purposes closer to the use of sources than to the use of experiments. The populations surveyed are not created by researchers, and they may not have exactly the right characteristics for data derived from them to answer the researchers' questions.

Disciplines in which surveys are used have well-developed methods for the collection and the analysis of survey data.

Data mining can reveal interesting patterns, but there are special risks. A large body of data is very likely to reveal some patterns, but they may not be significant. It is possible for researchers to pursue evidence for a conclusion that is suggested by a pattern, while they do not notice a lack of evidence that the pattern is significant. And it can be difficult to test hypotheses properly. But there are precautions that can be taken.

Chapter 8: Routes to Accounts

8.1: Introduction

Researchers need to reach accounts and individual claims by taking routes from evidence or from other starting points. They may deduce claims, or they may reach explanatory accounts by taking the good explanation route.

8.2: The deductive route

The deductive route encompasses both deductions and arguments that are almost as strong. The route is conspicuous in mathematics and the natural sciences.

Deduction from evidence can give considerable confidence in conclusions reached, so long as there is confidence that the evidence does not mislead.

The validity of arguments needs to be checked. The dialogical conception of deduction can provide reassurance.

Claims that are drawn from the existing corpus of the relevant discipline will often play an important role in use of the deductive route. We must ask whether premises that are drawn from the corpus have adequate support.

8.3: The good explanation route

The good explanation route involves a search for possible explanations, followed by appraisal of the candidates.

In appraisal, researchers can look for standard virtues of explanations. Other good signs include consilience, successful triangulation and multiple derivability.

The identification of single best explanations, or of good explanations, can support claims within explanantia.

Searches for good explanations must be wide-ranging, but they must also be guided. Evidence can be recharacterized in the search for good explanations.

In some disciplines, it is important to identify the meanings that people who are studied attributed to events, objects, social structures, social roles, and human character traits and actions. The identification of attributed meanings can allow researchers to give explanations that confer Verstehen.

The identification of attributed meanings requires careful consideration of both the detailed evidence and the overall

pictures that emerge of the psyches and the societies of the people studied. Researchers can obtain guidance from the hermeneutic tradition in the interpretation of texts. But there are some differences between the identification of meanings of texts and the identification of meanings that were attributed to non-textual items. There is also some risk of wrongly specifying attributed meanings.

There are various controls over use of the good explanation route. There is scrutiny of work by other researchers. The diversity of outlook of researchers is some protection against researchers being led astray by mistaken decisions as to how to approach phenomena. The influence of the existing corpus can provide some protection against going astray because good explanations have not been considered, or because explanations have not been ranked appropriately. And formal epistemology can provide some controls over processes of reasoning.

Chapter 9: Creators of Accounts

9.1: The complexity of the world

The complexity of the world means that researchers must work in groups and must rely on testimony. Independent appraisal of work is vital. New technologies can help researchers. Epistemic virtues are important.

9.2: Work in groups

Work in groups is in practice essential in many fields. It can also be argued to be an independently valid way to work, and not merely a practical development from individual work.

Groups can help to impose norms. They can also facilitate the sharing of ideas, although we may sometimes have

concerns about how consensus is reached when that sharing exposes disagreement.

Work in a group can help to impose the orthodoxy of a research community. That can allow great increases in productivity, but at the risk of neglecting unorthodox work that should be taken seriously, of losing some beneficial diversity in debate, and of groupthink.

9.3: Reliance on testimony

Researchers rely on testimony. We must consider both the content of testimony and the social aspect of its production and reception.

9.4: Independent appraisal

The independent appraisal of work done is a vital control. Criticism must be effective. Even when conditions that should help it to be effective are satisfied, we may have concerns. Independence is a matter of degree, rhetoric may influence appraisal, some work is never appraised carefully, and work that might show apparently interesting results to be due to chance may not get published.

Traditional peer review both checks that work submitted for publication meets a standard, and rations the available places in peer-reviewed journals. The rationing may concern us both because it may encourage researchers to make their results seem more exciting than they really are, and because it may block the peer-reviewed publication of work that would pose challenges to other work.

There are grounds for concern about the effectiveness of peer review. Reviewers may not have the time to review papers carefully, and they may be able to identify authors even when their names have been removed from papers.

There are now plenty of opportunities for open comment on work, in ways that make it easy for researchers to find comments. This should improve control over the process by which claims come to be accepted.

The ultimate form of control is to repeat work that researchers have done, to see whether the same results are obtained. But this is costly, and it is therefore only done in a modest proportion of cases.

9.5: New technologies

New technologies are transforming the way research is conducted.

Citation indices, sophisticated tools for conducting searches, and the standardization of terminology all help researchers to find the literature they need.

Specialized wikis can both supply information and help researchers to navigate the literature, but control over the quality of their contents is needed.

New technologies facilitate work in large, dispersed and open groups by making it easy to share material, receive contributions, and manage the production of statements of results to which all members may contribute.

Large, dispersed and open groups should be less exposed than small, geographically concentrated and closed groups to the risks that potentially productive but unorthodox work may be met with hostility or neglected, that diversity in debate may be reduced, and that groupthink may arise.

New technologies also facilitate the sharing of all of the information about a project with other researchers.

9.6: Epistemic virtues

If researchers exercise epistemic virtues, that will increase both their effectiveness and our confidence. In addition, it will help to make it safe for other researchers to make use of their work.

In work that leads to the making of claims, accuracy and completeness are particularly important. It is also important only to make claims when there is sufficient evidence for them, but not to make this demand so extreme that the relevant discipline cannot progress.

Imagination must be both disciplined and lively.

When claims are appraised, freedom from prejudice is important. Some well-established assumptions that are not recognized as prejudices may hinder progress. But some entrenched preferences play valuable roles in making disciplines productive.

Sound judgement in the choice of standards and methods of appraisal is important. The social sciences and the humanities use standards that differ from those used in the natural sciences. But contextualism is not relevant.

There are commercial and other risks to the integrity of research, and the virtues of researchers provide a line of defence.

Chapter 1

Disciplines and Claims

1.1 Our main question

Our field of study is work in the full range of academic disciplines, from mathematics and physics, through other natural and social sciences, right up to the humanities, disciplines such as history and the study of literature. Both traditional and new disciplines, and all of their sub-disciplines, fall within our scope. We shall use the word “discipline” to mean either a large discipline or a smaller sub-discipline.

Within any academic discipline, individuals will make claims. Some claims will gain the assent of the generality of expert researchers. We shall say that such claims are accepted. Our main question will be this:

- How should various general features of disciplines affect our confidence in the correctness of accepted claims?

"General features" will mean features such as the availability of deductive relationships between propositions, the precision of concepts and the types of argument used. They are general in the sense that they are not specific to particular disciplines.

The confidence in question is our confidence in claims in general, rather than our confidence in individual claims. It is the confidence of one who looks at work in a given discipline from outside that discipline.

We shall speak simply of confidence, or of confidence in relation to a particular discipline, or of confidence in claims of some type or other. All such expressions should be taken to refer to our confidence in the correctness of accepted claims. We shall speak of researchers assenting to claims, so as to distinguish assent by individual researchers from the acceptance that follows when assent is sufficiently widespread. And we shall speak simply of researchers, meaning expert researchers.

There are several elements in our main question that require explanation. We shall now give that explanation. For expository reasons, we shall cover elements in a different order from the order in which they appear in the question.

1.1.1 Claims

We shall consider various types of claim. A few claims are large-scale, for example claims that set out major theories or laws in the natural sciences, or claims that characterize whole economies or historical periods. Many claims are small-scale claims about points of detail. And some claims fall between these extremes. Many of the most interesting claims arise in the context of efforts to explain phenomena.

Our main source of claims to consider is the documents that give firsthand reports of the results of research – academic papers in all disciplines and sometimes, particularly in the social sciences and the humanities, monographs or more wide-ranging books. Only a modest proportion of the claims made in such documents eventually find comfortable homes in textbooks, which give secondhand reports of what researchers have discovered.

Academic papers in all empirical disciplines spend a lot of time describing the work done and reporting observations, or describing source material and reviewing earlier literature, and comparatively little time setting out conclusions. In the natural sciences, conclusions are often not preceded by forthright statements of claim like "We have discovered that ... " or "We have shown that ... ". Indeed, it is common for a paper to end with a section that is modestly headed "Discussion". But specific claims are still made, and our main question can be asked in relation to them. In the social sciences and the humanities, it is often more obvious that claims are being made, whether in papers or in those books that give firsthand reports. Moving away from empirical disciplines, we find that in mathematics it is most obvious of all that claims are being made, because papers supply proofs of theorems.

1.1.2 Correctness

We shall take it that there is such a thing as correctness, and that the goal of researchers is to make correct claims and avoid making incorrect claims. We shall now discuss both the notion of correctness and the goal of correctness. In sections 1.1.5.2 and 1.1.5.3 we shall discuss how it may be possible to have confidence that accepted claims are

correct given that outside mathematics, it is impossible to be absolutely certain of anything.

1.1.2.1 The notion of correctness

The notion of correctness is meant to be taken in a straightforward way. There are some things it is correct to say about the world, and other things it is incorrect to say. A claim may be regarded as correct even if researchers fear that it is only very close to correct, so long as the feared inaccuracy would not have serious consequences. And claims of some types may be rendered safe from falling a little short of correctness by including acknowledgements of imprecision, as in "The distance is 37.3m to within 1mm".

We do not intend to say anything that might require us to take a position on questions of realism or anti-realism, or on whether social concepts should be seen as mental constructs. We are interested in the claims that are made within disciplines, rather than in how those claims might be read. In the same vein, we shall not make any essential use of the concepts of truth, of truthlikeness (verisimilitude) or of knowledge, because we would like to steer clear of the controversies that are associated with those concepts.

We do however need it to be legitimate to regard claims as correct or incorrect. This does not mean that there must be verdicts on all claims. At any given time there may be many claims which researchers can formulate, but the status of which they cannot determine. All that matters is that on the whole researchers can expect that claims would be either correct or incorrect, and that the status of most claims could in principle be determined. There may still be some claims that it is impossible in principle to determine either to be correct or to be incorrect, given that a discipline

is set up in a certain way. Claims may for example be fundamental presuppositions of the discipline, or they may assign arbitrary values to free parameters.

The legitimacy of regarding claims as correct or as incorrect is essential because if that were not legitimate, we could not even try to answer our main question. We must therefore reject forms of relativism and constructivism that would be strong enough to deny that legitimacy.

Fortunately, there are good arguments against such strong positions.[1] Even if those arguments did not suffice to hold such strong positions at bay, we could get further assistance from the limited nature of our requirement. We only require that it should be legitimate to regard claims as correct or as incorrect from the general viewpoint of the discipline within which the claims are made. That general viewpoint will include an understanding of the sorts of entity to which reference may be made, the sorts of properties of entities that should be recognized, and the sorts of procedure that should be used to appraise claims. This acceptance of the general viewpoint of the relevant discipline for the sake of our argument excuses us from debate with relativists over whether alternative general viewpoints would be equally legitimate. A wide range of relativists should be able to put their concerns about alternative viewpoints to one side and engage with our argument on its own terms. It is perfectly reasonable to consider effects of features of disciplines on our confidence while not questioning the broad outlines of the relevant disciplines, even if one might wish to question those broad outlines as a separate exercise.

[1] See for example Boghossian, *Fear of Knowledge: Against Relativism and Constructivism*. For some technical arguments against relativism and against some of its underpinnings see Cappelen and Hawthorne, *Relativism and Monadic Truth*.

Correctness is the correctness of a claim, not the correctness of the act of making it or of assenting to it. For a claim to be correct, it must get things right. There might also be standards of correctness for acts of making claims or of assenting to them, standards which would lead us to think that such acts were correct if they were sensible, or appropriate, or the best that researchers could do in the circumstances. But we do not intend to be so generous when considering whether claims are correct.

1.1.2.2 The goal of correctness

It is manifestly the goal of researchers to make and assent to correct claims, and neither to make nor to assent to incorrect claims. If we seek to understand what goes on in academic disciplines, we must not overlook that fact. It is also perfectly right that this should be the goal of researchers. A vital source of pressure on them would be lost if it were not.[2]

There is a complication to acknowledge here. Thomas Nickles has argued for the importance of heuristic appraisal alongside epistemic appraisal.[3] Heuristic appraisal leads researchers to look kindly on claims which appear to have the potential to lead to useful further work. But having acknowledged that this form of appraisal may play a role, we shall not pursue the point. We need not do so because researchers can distinguish between the thought that a claim is correct and the thought that it may be useful. There is however a general point that when we consider whether researchers act appropriately in considering whether to

[2] Compare Price, "Truth as Convenient Friction", pages 169-170, on the importance of caring about truth.

[3] Nickles, "Heuristic Appraisal: Context of Discovery or Justification?"

assent to claims, we must be aware that their judgement may be affected by a preference for claims which hold out the promise of supporting future work. That could reduce our confidence.

In some disciplines, and particularly in the humanities and the social sciences, there is scope to debate whether the goal should be to make claims that could be regarded as correct. It may be argued that some claims are appropriate to make because they are supported to a high degree, or because they improve understanding, even if they clash, or might easily clash, with other equally worthy claims. We shall not pursue that line, because it is important to respect the observed practice of disciplines. In all disciplines, researchers write in ways which implicitly or explicitly assert the correctness of their claims. Even if researchers make contradictory claims, each claim is regarded as correct by those who make it.

Having said that, we need not have any difficulty with the simultaneous acceptance of different but compatible claims about the same phenomenon, or with the simultaneous endorsement of different but compatible explanations of the same phenomenon.

1.1.3 Expert researchers

We shall concentrate on the work and the views of expert researchers. Experts are not always right, but they make nearly all of the advances in their disciplines. Outside commentators and the wider public can only rarely make significant contributions or put experts right. There are issues connected with the funding of research, the application of research and the ways in which politicians obtain and act on expert advice, where it is important that

the wider public should be involved. There is also a strong case for ensuring that the process of research and its results are open to public view. That allows people both to see how research works, and to understand how the claims made are often less exciting than newspaper headlines would suggest. But when our interest is in the nature and the practice of disciplines, and in effects on the extent to which we may have confidence in accepted claims, we need to concentrate on what experts say and do.[4]

We shall be fairly liberal about whom we regard as an expert researcher. We shall include all the practitioners of a discipline who by virtue of their skills, understanding and work might well make worthwhile contributions, regardless of whether they have any institutional status. This is hardly a full definition, but we can generally recognize people who would qualify. We shall use the term "researcher" as a short form. When we use terms such as "physicists", "historians", or "social scientists" to refer to practitioners of specific disciplines, it is to be understood that they have the necessary expertise. We shall use the first person plural pronoun, "we" and "us", to pick out the author and the readers of this work as external commentators on disciplines.

[4] Weingart, "How Robust is 'Socially Robust Knowledge'?", shows how the view that claims made within science would benefit from some democratization of the process of science is simply muddle-headed. It may however be perfectly appropriate for researchers to enter into dialogue with people who are outside their own communities, when those outsiders have relevant expertise: Rolin, "Scientific Knowledge: A Stakeholder Theory", especially pages 70-76.

1.1.4 Assent and acceptance

Our main question speaks of accepted claims. Unanimous assent is not required for a claim to come to be accepted. Indeed, more than a few dissenters can often be tolerated. We shall not attempt to fix a tolerable percentage. And if a claim is not accepted, it will not follow that researchers assent to any alternative claim, such as the claim's contradictory. They may simply have no view.

It may be clear that a claim commands enough assent to have come to be accepted. Widespread assent may be explicit. Alternatively, and more commonly, there may be the explicit assent of a few researchers who have a special interest in the claim or special expertise in its topic, accompanied by the implicit assent of others who may rely on the claim, but who lack any special interest or special expertise. Implicit assent may be inferred from the fact that initial debate over a claim has died down, or from the fact that the claim is widely used in further research. But it may not be clear whether a claim has come to be accepted. The claim may not have attracted much discussion, or it may not have been much used.

There will often not be a clearly demarcated stage of putting a claim on trial. So we shall regard a claim as accepted if it has survived debate and is no longer regarded as controversial, or if actual or potential use of the claim by researchers other than those who made it does or would take place without any concern as to the claim's correctness.

Our notion of assent has something in common with the notion of acceptance that is set out by Mark Kaplan, although his notion relates primarily to individuals rather than to communities of researchers. For Kaplan, someone accepts a claim when she is willing to assert it in the

context of enquiry. As Kaplan says, this does not require either certainty or the achievement of some particular level of confidence. We do however expect that researchers will only explicitly assent to a claim when they have strong reasons to think that the claim is correct. This sets our notion of assent apart from Kaplan's notion of acceptance. He is prepared to countenance researchers being attracted to acceptance of a theory by its power, even if they recognize its improbability.[5]

We shall take it for granted that assent, and therefore acceptance, should depend on the state and content of the evidence that is available or can easily be obtained. In academic disciplines, evidence is largely made explicit. Moreover, when claims are debated, there are usually several participants who between them will be aware of most of the relevant literature. This means we need not be concerned with the worry about evidentialism that it may make the propriety of someone's believing a proposition depend too much on the evidence that she may happen to have or not to have forgotten.[6]

1.1.4.1 The generality of researchers

Our notion of the generality of researchers needs refinement in the social sciences and the humanities. In these disciplines, it is not uncommon to find large-scale disagreement

[5] For all of this paragraph see Kaplan, "Decision Theory and Epistemology", pages 451-453. Kaplan writes within an explicitly Bayesian context. That fact should forestall any objection that his notion of acceptance should be rejected out of hand because of its apparent disregard for the need to try hard only to assent to correct claims.

[6] For an outline of such worries see Mittag, "Evidentialism", pages 172-174.

between groups of researchers. Disagreements may be of two types, and it is disagreements of the second type that require us to refine our notion.

The first type comprises outright disagreements on specific claims about the primary subject matter of the discipline. Some researchers assent to claims that others contradict. Disagreements of this type may well mean that none of the claims in contention should be regarded as accepted.

The second type arises when researchers differ on the appropriate approach to the subject matter of the discipline. For example, some sociologists see individuals as primary. They see social phenomena as lacking any form of independent existence, and as to be explained in terms of the lives of individuals. Other sociologists see social phenomena as having some form of independent existence, as having primary roles in explanation, or both.[7] To take another example, some historians make extensive use of Marxist notions while others do not use them.[8]

This second type of disagreement presents a difficulty for us. When a disagreement of this type arises, there may still be claims that enjoy general assent and are regarded as important across the divide. But there are likely to be other claims that are directly about the objects of study and that enjoy general assent and are regarded as important by researchers on one side of the divide, while those on the other side tolerate them grudgingly and regard them as not getting to the nub of the matter. (This is not assent versus contradiction. That can happen, but in that case we could not regard the relevant claims as accepted in any case. We

[7] For such options see Zahle and Collin (eds.), *Rethinking the Individualism/Holism Debate: Essays in the Philosophy of Social Science*, chapter 1, sections 1.1 and 1.2.

[8] For the use of such notions and the impact of their use see Blackledge, *Reflections on the Marxist Theory of History*, chapter 1.

should treat cases of assent versus contradiction as examples of the first type of disagreement.)

It is claims that are welcomed enthusiastically on one side and tolerated grudgingly on the other side that give us a problem, even though there is no outright disagreement at that level. The reason is that there is likely to be outright disagreement at the next level up. Those on one side will assent to claims that certain explanations of phenomena are respectably strong, while those on the other side will contradict those claims.

For example, criminologists who take different approaches may agree that given criminals had certain thoughts before committing crimes, that they had certain traits, that their societies had certain structures and that their lives followed certain courses, but they may go on to explain crime in terms of individual choices, individual traits, social structures or social processes, with criminologists who adopt different approaches regarding different explanations as primary.[9] To take another example, historians with varying degrees of enthusiasm for Marxist thought (and also varying in other respects) have given markedly different accounts of the origins of the French Revolution – although the debate is driven partly by novel analyses of the evidence, and not solely by the varying overall stances of historians.[10]

The difficulty is this. Are we to regard such higher-level claims about which explanations are strong and worthwhile, claims that enjoy general assent only on one side of a divide, as debarred from being regarded as accepted, so that we cannot even ask our main question?

[9] Siegel, *Criminology: the Core*, chapters 4 to 7.

[10] Doyle, *Origins of the French Revolution*, chapters 1 to 3. Doyle's remark (on page 38) that Cobban could accept the research of Lefebvre and Soboul without accepting their overall approaches makes it clear that overall stances of historians have had a role to play.

We need not go so far. Instead, we can regard some claims as accepted relative to adoption of a certain approach to the subject matter of the discipline. We can then ask how our confidence in those claims should be affected by general features of the discipline, on the assumption that the approach in question is appropriate. When we discuss how general features of disciplines should affect our confidence, we shall take it that any approach that would need to be assumed in order for it to be possible to regard the claims as accepted is appropriate. (We shall however only do so for the sake of argument, in order to examine the effects of general features. There will be no suggestion that the approach should in fact be endorsed.)

This generosity must however be limited, in three different ways. The first limit is that any approach that is to be taken for granted must not be so specific as to steer researchers toward particular claims and away from other claims, from among the claims that would be made legitimate by adoption of the approach. Approaches that are to be taken for granted must therefore be general rather than specific. The second limit is that generosity must not extend to approaches that are too idiosyncratic. It is reassuring if an approach has been adopted by a substantial number of researchers whose backgrounds are diverse. It is also reassuring if most of those researchers assent to many of the claims to which those who adopt other approaches also assent. (They will however not assent to all such claims, if their approach is significantly different from other approaches.) If such reassuring signs are unavailable, the approach in question may be thought so odd that claims to which only researchers who adopt it assent are automatically suspect. The third limit is that generosity should not extend to approaches that involve the uncritical use of suppositions which are such that researchers from a wide variety of schools of thought would expect any use

of those suppositions to be subject to critical appraisal.[11] Whether an approach oversteps any of these limits to generosity must be a matter for judgement in each case.

1.1.5 Confidence

1.1.5.1 Having confidence in general

Our main question concerns our confidence in the correctness of accepted claims. We do not ask what features of a discipline might make us fully confident that all of the accepted claims within it were correct. That would be too demanding an aspiration. Nor do we ask what features might give us confidence that some particular proportion of accepted claims were correct. Rather, we ask about the legitimate influence of features on the level of our confidence in general. The word "should" in our main question indicates that we are interested in how we ought to think, rather than in how we may happen to think. But we shall not spell this out repeatedly. When we remark that some feature may influence our confidence, this should be taken to mean that it may legitimately influence our confidence.

Our confidence may be influenced by features of disciplines, at whatever level the confidence may happen to be. Influences will be of interest to us whether we are fully confident that nearly all accepted claims are correct, have

[11] An example would be the uncritical use of assumptions that are contained within or that underpin rational-choice theory. For those assumptions and reasons why they need to be examined critically see Reiss, *Philosophy of Economics: A Contemporary Introduction*, chapter 3. For a wide-ranging discussion of conceptual issues surrounding rational-choice theory see Pizzorno, "Rational Choice".

doubts about the correctness of a substantial proportion of accepted claims, or are at some intermediate position.

The fact that we are concerned with how things are on the whole is the reason why we talk about levels of confidence in claims, rather than levels of justification for claims. Justification is enjoyed by claims individually, but we can speak of a level of confidence in claims in general.

The use of the imprecise words "on the whole" in the preceding paragraph is deliberate. While high confidence would indicate a belief that most accepted claims were correct, and low confidence would indicate a fear that a substantial proportion were incorrect, we shall not pretend that proportions could be determined mathematically. Fortunately, we shall not need such determinations. But if we were to give a formula to compute an overall level of confidence from levels of confidence in individual claims, it would be some kind of average of those individual levels.

1.1.5.2 The risk of error

Research in academic disciplines is a journey into the unknown. Researchers have no way to transcend the evidence which leads them to make or assent to claims, in order to compare their claims with claims that are independently certified to be correct. As has been remarked, the conduct of a piece of research is like an epistemic lottery. Researchers hope to be rewarded with correct belief, but even after the event they cannot be quite sure they have won that prize.[12] Given that this is so, how could we, even in principle, have confidence that accepted claims were indeed correct?

[12] On the lottery image see Fallis, "Attitudes Toward Epistemic Risk and the Value of Experiments", section 2.

A general response is that our confidence does not require researchers to have certainty. We may have confidence if we are aware that researchers have substantial positive support for claims, and that they have no specific reasons to doubt those claims despite their having searched for reasons for doubt. A general sceptical worry, not related to specific reasons for doubt, is not to be allowed to destroy confidence.

A more specific response is that researchers happily make and assent to claims without their having complete certainty. Doing so is an essential part of the practice of academic disciplines. This should not destroy our confidence. The practice is not a free-for-all, but one in which norms must be observed.[13]

One perfectly standard circumstance in which researchers assent to a claim without certainty arises when they would have good reason to regard the claim as correct, or as approximately correct, if they were to take the current body of well-supported claims to be correct. If for example they had an existing well-supported theory, and they considered a new claim that was well-supported by reference to a combination of that theory and the new evidence which had prompted the claim, it would be entirely rational for them to say "If our theory is correct, then this claim is very likely to be correct or at least approximately correct". Given the support for the theory, it would then be rational for them to assent to the claim. Such a pattern of reasoning is not often made explicit, because the theory is simply taken for granted, but it is still a perfectly sensible way to reason.

[13] As well as norms appropriate to the relevant discipline, there are constraints on the natures of the epistemic utility functions that should govern the practice. See Myrvold, "Epistemic Values and the Value of Learning".

1.1.5.3 The risk of transformation

Beyond the risk of specific errors, there is another risk. Experience shows that there is always a risk of some far-reaching transformation, such as the shift from classical to quantum mechanics, or the shift away from thinking of the early mediaeval period as a period of uniform decline into a dark age.[14] Following such a transformation, some existing claims may need to be replaced by new claims, although the new claims are likely to capture all or most of the phenomena that the old claims captured, perhaps along with some additional phenomena. Other existing claims may survive, but they may come to be seen in a new light.

Given the risk of far-reaching transformations, it might seem that confidence in accepted claims would always be unwarranted. But we do not wish to close the discussion so abruptly. We shall take "confidence" to mean the confidence that we may have if we disregard unspecified risks of far-reaching transformations. What may undermine the confidence we wish to identify is not awareness of such unspecified risks, but awareness that researchers sometimes assent to claims without giving due weight to specified risks, such as the risk of errors in measurement or the risk that a text which is only available in late copies may have been corrupted over the centuries. We shall however allow that confidence may be undermined by awareness that researchers might assent to claims without giving due weight to a risk that some specified far-reaching transformation would shortly be made. We must also allow that it is a matter of judgement whether a possibility of change is an unspecified risk that we should disregard for

[14] For the movement away from seeing uniform decline into a dark age, and the ways in which new work has shown that the picture was more complex and in several respects brighter, see Heather, "Late Antiquity and the Early Medieval West".

the purposes of our argument, or a reasonably well-specified risk that should diminish our confidence if researchers do not allow for it. Not all transformations are as dramatic as the advent of quantum mechanics. Some of them have been modest enough that we should seriously consider whether their possibility might have presented well-specified risks.[15]

1.1.5.4 Positive and negative considerations

If claims in a discipline only come to be accepted when they enjoy ample positive support, that should increase our confidence. Positive support may come from a claim's being entailed by other strongly supported claims, from its following from such claims in some way that is weaker than entailment, from pieces of evidence that speak directly in favour of the claim, or from the fact that the claim makes sense of a range of evidence more effectively than alternative claims would do.

It is important to recognize the existence of positive support. Within all disciplines, academic books and papers are replete with evidence in favour of the claims made. Our recognition of positive support arguably sets us against Karl Popper and some heirs to his line of thought, but the fact that academic books and papers set out positive support is not to be ignored.[16] It is also perfectly possible to articulate

[15] Some examples of relatively modest transformations can be found in González (ed.), *Conceptual Revolutions: From Cognitive Science to Medicine*.

[16] Popper sets his face against the verification of theories, in any sense of verification that would go beyond their corroboration through having withstood attempts to falsify them, in *The Logic of Scientific Discovery*, chapter 10. Popper's initial focus of attention was the natural sciences. Appropriately for those disciplines, he saw actual falsification as requiring reproducible effects that would refute a theory: *The Logic of Scientific Discovery*, chapter 4, section 22. The

a view of the interaction between theories and experimental data that offers a more positive prospect than the view that the main business of science is to identify incorrect theories and discard them.[17] And the notion of positive support is lent respectability by the scope to formalize it within confirmation theory. While scope to formalize does not demonstrate the worth of a notion conclusively, it does strongly suggest that the notion is far from vacuous.[18] We shall return to the legitimacy of the notion of positive support in section 6.1.1.2.

Confidence can be increased by evidence that researchers check for reasons not to make claims before making them or assenting to them. Ideally, those who consider whether to assent to claims should not merely note that no such reasons have come to light. They should also see whether there are checks that could be made but have not yet been made.

1.1.5.5 Support, acceptance and confidence

We shall not always speak explicitly of effects on confidence. We shall sometimes discuss whether claims should receive

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extension of his views beyond the natural sciences required a shift from falsification to criticism, which he placed under the banner of "critical rationalism": Popper, *The Open Society and its Enemies*, chapter 24, sections 1 to 3. The same use of criticism rather than falsification is visible in the work of Hans Albert: Albert, *Traktat über kritische Vernunft* (*Treatise on Critical Reason*), particularly chapter 4, section 14 and chapter 7, section 28.

[17] Kuipers, *Structures in Science: Heuristic Patterns Based on Cognitive Structures*, section 8.2.

[18] For an outline of the Bayesian formalization see Hawthorne, "Bayesian Confirmation Theory". For an analysis of why it is productive to take the decisions that confirmation theory recommends see Huber, "What is the Point of Confirmation?".

assent and thereby come to be accepted, or whether they would be well-supported, given their types and the circumstances in which they were made. But the chain of implications is straightforward. If claims are generally well-supported, we may have confidence that they are by and large correct. On the other hand, we should not have that confidence when claims are not generally well-supported, even though some of the claims may be correct. Rather than examining the support for claims directly, we may be guided by the practice of researchers. If we can see that on the whole they only assent to well-supported claims, then we may have confidence in accepted claims in general.

We have here made a strong connection between support and confidence. We must add two points in order to justify this connection. The first point is that it is a connection in relation to claims in general, not in relation to each claim considered individually. We acknowledge that some well-supported claims will be incorrect. The second point is that we are using the notion of confidence that has been set out here. We may have confidence despite general sceptical worries, and despite awareness of the risk of unspecified future transformations.

Processes by which claims come to be made

Before claims win assent and come to be accepted, they must be made, and what happens at this stage can affect our confidence. If norms that govern the making of claims require claims to be well-supported, that can be a very useful source of confidence. Incorrect claims are more likely than correct claims to lack adequate support. They misrepresent the world in some way, so there is likely to be a shortage of evidence in their favour. A requirement for adequate support will therefore reduce the number of

incorrect claims that are made. Since some proportion of incorrect claims will slip through the controls that are imposed after claims are made, reducing the number that are made will reduce the number that come to be accepted. So our confidence will be increased by reasons to think that few inadequately supported claims are made. The risk of inadequately supported claims slipping through is particularly high in relation to claims that do not attract much discussion but just lie in the literature, and are regarded as respectable mainly because of the talents of the authors of the relevant papers and the fact that those papers were peer-reviewed.

1.1.5.6 Our confidence as outsiders

It may be asked why our main question is couched in terms of the confidence that we may have as philosophers standing outside disciplines, rather than our using the more direct approach of asking whether researchers manage to make and assent to correct claims and not incorrect ones. There are two steps back from the direct approach, a step back from researchers to outsiders and a step back from correctness to confidence.

We take the step back from researchers to outsiders for the following reason. Researchers naturally believe that their disciplines are practised in the right ways, and they naturally think that accepted claims are correct. If they did not think these things, they would work in different ways and they would not assent to the claims in question. (Some researchers may dispute some accepted claims, but any given accepted claim will generally be taken to be correct.) We need an external position from which we are not committed to the practices or the accepted claims of whatever discipline is under consideration, in order to ask

what grounds we may have for thinking that the practices really are sound and that accepted claims are correct. By taking the step back from researchers to outsiders, we make the external nature of our position explicit. We shall however accept the general natures of disciplines. We shall not try to think outside the frameworks that are provided by those general natures.

We take the step back from correctness to confidence because we take the step back from researchers to outsiders. Most of the time, researchers are the only people who are in a position to assess the correctness of claims. All that outsiders can do is have more or less confidence that the researchers are doing their jobs well.

Although there are these reasons for setting a rather indirect question we shall, as we noted in section 1.1.5.5, sometimes cut short our pursuit of a chain that leads all the way to our confidence. We shall sometimes only ask whether claims should receive assent and come to be accepted, or whether they are well-supported. The rest of the chain need not be mentioned when the effect on our confidence is obvious.

The content of our confidence is to be derived from how researchers in the relevant discipline regard correct claims. If it is normal in a discipline to regard correct claims as stating straightforward facts about the world, with no sense that they primarily give interpretations of the world, then confidence amounts to confidence that accepted claims do state facts. If it is normal to regard correct claims as giving interpretations of the highest quality, then confidence amounts to confidence that accepted claims do give interpretations of the highest quality. The same would apply to any other way in which correct claims might be regarded. The move from researchers to outsiders does not

entail a move from a sophisticated view of correctness to a naive view.

1.1.6 Features of disciplines

The notion of features of disciplines has a wide scope. It includes features of disciplines as wholes, of explanations that are given within them, of individual propositions, of relationships between propositions, of concepts that are used, of practices and of researchers. Thus although our focus is on claims, we are not by any means limited to considering explicit tests of claims.

The scope of our notion of features might seem to be too wide for the expression "features of disciplines", and there is a sense in which this expression is a convenient piece of shorthand. But it is perfectly appropriate to say things like "It is a feature of this discipline that there are many deductive relationships between propositions", or "It is a feature of this discipline that new claims are tested experimentally".

As our main question indicates, we shall be interested in general features of disciplines. These are features that may be exhibited by several disciplines, rather than features that are limited to one or two disciplines.

In addition to responding directly to our main question, we shall at various points explore the reasons why disciplines have certain features. The fruits of these explorations may not contribute directly to the responses we can make to our main question, but they should deepen our understanding of why our responses are what they are.

1.1.6.1 Assessing the effects of features

Our main question asks about effects of features of disciplines on our confidence. As we noted in section 1.1.5.1, this is confidence in accepted claims in general, not confidence in specific claims.

Confidence in claims in general looks like a statistical notion, but we shall not attempt to compute levels of confidence for disciplines. It would be impossible to make such computations. We shall only say whether features increase or decrease confidence.

We shall speak of favourable effects and adverse effects. A feature has a favourable effect if its presence tends to increase confidence, and an adverse effect if its presence tends to decrease confidence.

Finally, we shall not attempt to survey all features of disciplines that may affect our confidence. A comprehensive survey would be impractically large. It would also involve a great deal of overlap between discussions of related features.

1.1.6.2 The use of examples

We shall use examples of features that are drawn from a wide range of disciplines. The examples illustrate points, rather than proving them. They show that certain features exist, but not how widespread those features may be. We have not conducted systematic surveys, whether within individual disciplines or across disciplines. Having said that, examples were generally found quickly by conducting web searches using obvious phrases. The fact that they were found so easily suggests that the features exemplified are not rare.

Most of the examples are rather humdrum, drawn from small-scale work that has not caused any earthquakes in disciplines. The examples therefore come from widely scattered parts of disciplines. They do not give full pictures of their disciplines. But the choice of humdrum examples is deliberate. Most academic work is small-scale, filling in little gaps and making modest advances. If we concentrated on large-scale work that could indicate the overall shapes of disciplines, we would misrepresent the work of researchers. We would also paint a misleading picture if we concentrated on the rare occasions when new work transformed disciplines.[19] Since our interest is in the factors that should influence our confidence in the bulk of the claims that come to be accepted, it is important that we avoid such pitfalls.

1.2 The corpus

There is a set of claims for each discipline that holds a special interest for us. This set comprises the claims that are made within or are otherwise relevant to the discipline, and to which the great majority of researchers within the discipline assent. We shall call this set of claims the discipline's corpus. The union of all disciplines' corpora is *the* corpus. But when we discuss work within a discipline we shall simply refer to the corpus, meaning the corpus of that discipline, so long as there is no scope for ambiguity.

The corpus of a discipline includes not only claims about the objects of study, but also claims that certain methods

[19] Thomas Kuhn remarked on the danger of painting a misleading picture of scientific work in this way when he commented on Karl Popper's work: Kuhn, "Logic of Discovery or Psychology of Research?", pages 5-7.

are reliable. Such claims are often based on claims about the objects of study, as for example when physicists use claims about how particles interact with the rest of the world to show that certain methods are good ways to detect particles.[20] However, when claims about methods give rise to imperatives that dictate how researchers ought to work, those imperatives are norms. (We shall discuss norms in section 1.3, and again in sections 2.1.1.2 and 6.1.) For convenience, we shall regard norms as outside the corpus.

A claim in the corpus must be one to which the great majority of researchers assent explicitly, or one to which they would assent if they were asked. If too many researchers would either dispute a claim or decline to decide for or against it, it will fall outside the corpus. And the requirement for the assent of the great majority means that not every claim that is accepted in the sense we gave in section 1.1.4 will fall within the corpus.

We may identify the corpus of a discipline well enough. But the membership of any discipline's corpus will not be fully determinable. Fortunately, this will not affect our argument. All we need is that it will usually be clear whether a given claim is inside or outside the corpus.

It is important to identify the claims that represent the consensus of researchers, because those are claims on which researchers will tend to rely when they advance their disciplines. Reliance on the consensus of researchers is not subject to the same objections as reliance on the opinion of a single researcher who offers his own view, rather than reporting what researchers generally think.[21]

[20] Grupen, "Physics of Particle Detection".

[21] For those objections see Mizrahi, "Why Arguments from Expert Opinion are Weak Arguments". On page 61, Mizrahi explicitly distinguishes his target from the consensus of researchers.

The corpus plays vital roles not only when researchers draw conclusions by combining new evidence with claims in the corpus, but also when they make the most of existing evidence by understanding what might be inferred from it. This second situation in which the corpus matters has been highlighted by John Norton's material theory of induction. To borrow his example, researchers can reason from the melting points of some samples of bismuth to the melting point of bismuth in general, because the relevant corpus includes the information that the melting points of elements are usually the same from one sample to another.[22] We may add that the corpus includes a detailed physical and chemical theory which explains the general uniformity of melting points of samples of a given element, although it is arguable that observations of melting points, made in advance of formulation of the theory, would have sufficed to make legitimate a claim that elements tended to have uniform melting points.

The corpus of a discipline is not limited to claims that have their origins within the discipline. It may very well include claims that have roles to play within the discipline but that come from other disciplines. For example, many claims within physics are relevant in chemistry and biology, and therefore fall within the corpora of those disciplines, even though the claims remain the property of physics.

This phenomenon draws our attention to the question of which parts of a discipline's corpus are open to change as a result of work within the discipline. The normal presumption is that work within a discipline can change

[22] Norton, "A Material Theory of Induction", pages 649-650. For applications of Norton's theory see Norton, "History of Science and the Material Theory of Induction: Einstein's Quanta, Mercury's Perihelion". For some objections see Ducheyne, "Some Worries for Norton's Material Theory of Induction".

those parts of its corpus that are its property, but not those parts that belong to other disciplines. Thus chemistry and biology must, on the whole, take the parts of their corpora that belong to physics as given. On the other hand, a change to the corpus of physics may very well have effects in the corpora of chemistry and biology. The rule is not absolute. It is for example conceivable that a difficulty in chemistry would require a change in physics. But such cases are rare. We must also acknowledge that the ownership of claims is sometimes shared, whether they are claims inside or outside the corpora of disciplines. For example, the claim that a large increase in the money supply is in general inflationary belongs as much to history as to economic theory.[23] But the scope for shared ownership, or for any indeterminacy of ownership, will not affect the progress of our argument.

We shall speak of the content of a discipline. This will be the content of those claims that are within its corpus and that are its property. We shall speak of claims constituting parts of the content of a discipline as a short way of saying that the contents of claims constitute parts of that content.

We shall also refer to the central content of a discipline. This will be the part of the content that is given by claims which are significant across much of the discipline, and which give the discipline its character. The notion of central content is only loosely defined, but a loose definition will suffice for our purposes.

[23] A paper that uses both the perspective of history and the perspective of economic theory to analyse the same facts is Munro, *The Monetary Origins of the 'Price Revolution': South German Silver Mining, Merchant-Banking, and Venetian Commerce, 1470-1540.*

1.3 Norms

The norms of disciplines set standards of work. There is a wide range of norms, some of very general application and some specific to particular disciplines. Here is a non-exhaustive list of types of norm, given so as to indicate the breadth and the importance of the notion.

- Norms that are based on general epistemic considerations. Examples are a norm of preferring simple theories because holding that preference tends to promote efficiency in convergence on correct claims, and a norm of seeking new evidence rather than resting content with the current body of evidence and conclusions.[24]

- Norms of epistemic toleration, requiring researchers to give serious consideration to claims with which they disagree and to maintain a dialogue with the proponents of those claims.[25]

- Norms of argument, including norms that may require steps in argument to be deductive or to come close to being deductive. In mathematics, deduction is required. In physics and chemistry, deduction or something close to it is an aspiration: it may at least be possible to show how claims are woven tightly together by deductive relationships, even when it is not possible to give a deductive structure that expresses a whole theory. In the social sciences and

[24] For the preference for simplicity see Kelly, "How Simplicity Helps You Find the Truth Without Pointing At It". For the norm of seeking new evidence see Oddie, "Conditionalization, Cogency, and Cognitive Value".

[25] Straßer, Šešelja and Wieland, "Withstanding Tensions: Scientific Disagreement and Epistemic Tolerance", sections 5 to 8.

the humanities, deduction may find its main role (but not its only role) in working out relationships between pieces of evidence and detailed claims, as when a piece of evidence leads researchers to deduce that some claim is incorrect. All disciplines also have norms of clarity and precision in argument, although such norms are likely to be difficult to codify.

- Norms of the range of material that should be considered. For example, it is important to review the existing literature so that new research may be well-directed.[26]

- The norm of taking care to consider the natures of items of source material, the circumstances in which they were created and, where relevant, the intentions of authors, in order not to misinterpret sources. This is a very general norm, but it can issue in specific advice for particular types of source.[27]

- Norms of experimental design and control.[28]

- Norms for making statistical inferences and for testing hypotheses. Different levels of stringency are required in different disciplines. In particle physics, for example, there is an exceptionally demanding norm that a discovery may only be claimed when the 5 sigma level has been achieved. This corresponds to

[26] Boote and Beile, "Scholars Before Researchers: On the Centrality of the Dissertation Literature Review in Research Preparation", pages 3-4.

[27] Examples of specific advice for various types of source can be found in Dobson and Ziemann (eds.), *Reading Primary Sources: The Interpretation of Texts from Nineteenth- and Twentieth-Century History*.

[28] We shall give some references when we discuss such norms in section 2.1.1.2.

> $p < 3 \times 10^{-7}$ in a one-tailed test.[29] In social research, the demand may be only $p < 0.05$, although it can be stricter, for example $p < 0.01$.[30] Another norm relates to the selection of methods when making multiple comparisons. The norm is to select methods that will reduce the risk of error, while still allowing conclusions to be reached. Such a norm would not specify the precise selection of methods or of levels of stringency for testing hypotheses, because the area is complex and there is scope for different views as to what would be appropriate.[31] Having said that, there is no shortage of detailed guidance on how to make inferences from data and how to test hypotheses (in general, not just for multiple comparisons).[32]

Researchers observe norms with a view to making and assenting to claims that are correct, and with a view to not making and not assenting to other claims. Moreover, the norms do appear to do well at their job, at least in circumstances that are not peculiar. But even though norms

[29] The 5 sigma level has come to public attention as a result of the search for the Higgs boson. p is the probability that observations at least as extreme as those actually made would be made if the null hypothesis (in that case the non-existence of a particle of the relevant type) were correct. For announcements of results see ATLAS Collaboration, “Observation of a New Particle in the Search for the Standard Model Higgs Boson with the ATLAS Detector at the LHC”, section 10; CMS Collaboration, “Observation of a New Boson at a Mass of 125 GeV with the CMS Experiment at the LHC”, section 8.

[30] Bryman, *Social Research Methods*, page 348.

[31] An idea of the complexity of the area and of the scope for choice may be gained from Howell, *Statistical Methods for Psychology*, chapter 12 (section 12.8 discusses making choices); Dickhaus, *Simultaneous Statistical Inference With Applications in the Life Sciences*.

[32] To take just one example of a discipline, the extent of available guidance in psychology can be seen from the chapters in the two substantial volumes of Little (ed.), *The Oxford Handbook of Quantitative Methods*.

appear to do a good job, we can still ask what roles norms play and whether they have any weaknesses. Norms are among the features of disciplines, the effects of which are the concern of our main question.

Some of the norms that have substantial effects on how researchers in a given discipline work, along with some of the central content of the discipline, may form a research programme in the sense identified by Larry Laudan. Such a programme is a way of working that directs researchers and that is likely to endure for longer than individual theories, although it may eventually change.[33]

We shall return to norms later. We shall discuss their nature in more detail in section 2.1.1.2, and their role in giving confidence in section 6.1.

1.4 Differences

Disciplines differ from one another in several respects. For example, they differ in the extents to which:

- useful deductive relationships between propositions, or relationships that are almost as strong, are available;
- certain claims within the corpus impose significant constraints on what other claims may be made across the discipline (we shall call such claims "pervasive claims");
- claims that are made are straightforwardly falsifiable.

[33] Laudan, *Beyond Positivism and Relativism: Theory, Method, and Evidence*, pages 83-85.

We shall use such differences to construct a scale of disciplines in section 1.5, and we shall return to the idea of a scale in section 2.3. Features that differ as between disciplines will make repeated appearances as our enquiry progresses. But we shall make some preliminary remarks on deductive relationships and pervasive claims now, in order to support the scene-setting that we shall do in section 1.5 and in chapter 2.

1.4.1 Deductive relationships

Relationships between propositions may be deductive or non-deductive. Relationships can transmit support from some claims to others, so the availability of different types of relationship can have a considerable effect on our confidence.

We shall be concerned with relationships that researchers find useful. We could enquire into relationships between members of any pair or larger set of propositions, but relationships between the members of most sets would be uninteresting, so we shall not consider random selections of propositions. We do not limit ourselves to considering relationships of great significance, but we do limit ourselves to relationships that might play roles in worthwhile arguments or claims. We shall take this restriction as read from now on.

Our notion of deduction will be the broad notion that one would acquire from reading an introductory textbook on logic. It will cover, and will not discriminate between, the various more precise proof-theoretic, model-theoretic, semantic and epistemic notions that emerge on a more

refined analysis.[34]

It is rare for a single proposition or a small set of propositions to entail worthwhile conclusions. The antecedent of a deduction must usually include a large background set of propositions, which may amount to a substantial theory. In practice researchers may refer to deductive relationships between individual propositions, without mentioning the background that is needed to make the relationships deductive, because the background is taken for granted. When conducting our type of enquiry, we should frequently remind ourselves of the fact that the background is taken for granted. But there is no reason why researchers should do so.

It is important that the object of our attention at this point is relationships between propositions, and not the independent status of the propositions themselves. There is no expectation that any proposition outside pure mathematics could be established as correct merely by sitting in an armchair and playing with logic. But it is perfectly possible to establish that some propositions entail others. For example, the conservation of energy entails that perpetual motion machines of the first kind, machines which do work indefinitely without an external source of energy, are impossible.

[34] For an outline of some of these notions see Shapiro, "Varieties of Pluralism and Relativism for Logic". (In listing options on page 531, Shapiro gives a proof-theoretic notion as the second of his two epistemic notions.) The uses to which we shall see deductive relationships being put will be mainly uses in working forward from premises to conclusions, uses that Shapiro associates with a proof-theoretic approach rather than with a model-theoretic approach (page 533). But that does not require us to narrow our notion of deduction to a proof-theoretic notion.

The formulation and manipulation of propositions using the apparatus of formal logic would make it easy to identify deductive relationships reliably, but such formulation is rarely feasible, and the reliable identification of deductive relationships is harder in its absence. Researchers need to be aware of the danger of thinking they have identified deductive relationships through the use of a relatively informal language, such as the normal language of chemistry or economics, when they have not in fact done so.

Despite such concerns, we can extend the notion of a deductive relationship to propositions that are expressed informally. The extension is reflected in everyday assessments of whether relationships are deductive. It is the notion of deductive relationship that is used within logical systems, together with the natural extension of that notion to relatively informal languages, that we shall have in mind when we speak of deductive relationships.

We shall not regard all non-deductive relationships as radically different from deductive relationships. We shall recognize that some non-deductive relationships are almost as strong as deductive relationships, while others are much weaker. A claim that is well-supported provides support of equal quality for its deductive consequences. It can also provide a high level of support for consequences that are not deductive but that follow from it by virtue of relationships that are almost as strong. We shall say that a relationship is almost as strong as a deductive relationship if the antecedent's holding makes it exceedingly likely that the consequent will hold. We shall not try to make this notion more precise, because all we will need for our purposes is a recognition that some non-deductive relationships can be almost as good as deductive ones when the task is to show how some claims support other claims. And we shall not try to decide between different interpretations of

the words "exceedingly likely". We shall take those words to encompass both the consequent's holding on the overwhelming majority of actual or hypothetical occasions on which the antecedent holds or would hold, and researchers justifiably attributing a very high subjective probability to the consequent's holding when the antecedent is taken to hold.

We shall explore the significance of deductive and non-deductive relationships in chapter 3.

1.4.2 Pervasive claims

The list of differences between disciplines in section 1.4 mentions pervasive claims. These are claims within the corpus of a discipline that impose significant constraints on what other claims may be made across the discipline. Examples include:

- mathematical axioms;
- claims implicitly made by mathematical definitions, that entities of certain types have certain properties;
- physical laws of wide application, such as the principle of stationary action and the laws of thermodynamics;
- in biochemistry, the claim that proteins are made up of amino acid residues;
- in economics, the Von Neumann-Morgenstern utility theorem (although economists disagree about how useful this theorem is in the study of real economic agents).

Pervasive claims may sit at the foundations of systems of claims, as axioms do. We shall however allow for pervasive claims that are not in such foundational positions, but that simply impose constraints across their disciplines.

We shall consider pervasive claims in chapter 4. When the making of claims in a discipline is severely constrained by pervasive claims, that can have a favourable effect on our confidence. It can do so because it makes the requirement for new claims to fit in with the corpus a demanding one. In addition, the presence of pervasive claims can increase our confidence because their use in several contexts means that they are likely to have been well-tested. Their use in any one context can then lend some support to other claims in that context which are wholly or partly derived from them. Even if a claim is not derived from a pervasive claim, it may gain some support from the fact that it comports well with such a claim.

1.5 A scale of disciplines

The scope for disciplines to differ in the ways that we indicated in section 1.4 becomes clear as soon as we survey the range of academic disciplines. When we do so, the idea of a scale of disciplines naturally arises. The theme of a scale from lower disciplines to higher ones will recur throughout this work. Mathematics and physics will be placed at the low end and the humanities at the high end. "Low" and "high" are not terms of relative esteem. They are only used to allow us to distinguish directions. Moreover, the ordering will be neither total nor entirely determinate.

What follows here is only a preliminary discussion. We shall be able to say more about the ordering of disciplines on a

scale in section 2.3. We shall see there that disciplines could be placed in order on the basis of different types of feature, perhaps giving rise to slightly different orders. Nonetheless, we shall mostly speak of *the* scale. This will be safe for two reasons. First, the general picture we shall paint will be safe from variation between scales because all plausible scales would be much the same. And second, we shall not make any argument that would depend on the order in which specific disciplines were placed except when all plausible scales would place them in the same order.

1.5.1 Mathematics

Mathematics must be placed in a class of its own. On the one hand it is a pure discipline, an exercise in the manipulation of symbols that represent abstract structures and their features. The rules of manipulation and the validity of the results obtained are entirely independent of the nature of the physical world. (We shall return to this independence in section 4.2.3.) On the other hand, mathematics is an essential part of the languages of many of the disciplines that are concerned with the physical world. In physics, it is an overwhelming part of the discipline's language. Even in disciplines that lie far up the scale, such as economics and some types of history, mathematics can have a significant role to play in characterizing phenomena, and in deriving and expressing results.

When we consider the different extents to which disciplines may exhibit the characteristics we have noted, we find that mathematics is at an extreme. Relationships between propositions are overwhelmingly deductive, and those relationships tie the whole discipline together. The role of pervasive claims is played by axioms, by the implicit

claims that are made by certain definitions, and by some theorems.[35] These pervasive claims tightly constrain the options for making other claims. And the claims that are made are straightforwardly falsifiable, although the method of falsification is one of pure reasoning rather than an empirical method.

1.5.2 The natural sciences

We shall take the natural sciences to include physics, chemistry, biology, zoology and parts of psychology, particularly the more neurologically oriented parts. This is not an exhaustive list. Scientific disciplines and sub-disciplines have proliferated over the past century, and it would be impractical to list them all. Nor shall we attempt to give a general definition of the natural sciences, or demarcate them sharply from other disciplines. Rather, physics, chemistry and biology are clear examples of natural sciences, and the presence or absence of family resemblances will suffice to classify most other disciplines as within or outside the class of natural sciences. Our argument will not depend on our being able to give a fully determinable extension to the concept of a natural science.

When we transfer our attention from mathematics to the natural sciences, we find a new constraint. Claims must reflect the nature of the physical world. Empirical evidence dominates everything. The finest theory must be discarded if it conflicts with experimental results. While the large presence of claims of a mathematical nature means that some of the characteristics of mathematics

[35] Choices of definitions can be very significant: Tappenden, “Mathematical Concepts and Definitions”; Tappenden, “Mathematical Concepts: Fruitfulness and Naturalness”.

are exhibited in the natural sciences, the intrusion of the physical world means that those characteristics cannot be exhibited in their purest forms. And the extent to which those characteristics are exhibited diminishes as we move up the scale from physics through chemistry to the life sciences.

To start with physics, there are plenty of deductive relationships and relationships that are almost as strong. But they do not come anywhere close to structuring the whole discipline, or even substantial parts of it, as bodies of propositions that are deduced systematically from modest sets of axioms.[36] It is not surprising that Paul Hoyningen-Huene, when he proposes systematicity as the mark of scientific knowledge, only expects it to be more systematic than other kinds of knowledge. He does not expect it to be perfectly systematic.[37] Moreover, physicists take liberties with the standards of deduction on which mathematics prides itself.[38] All of these facts make it not wholly surprising that inconsistency can be seen to have a place in the scientific enterprise which goes beyond that of merely being a spur to its own elimination.[39]

[36] This lack of wide-ranging deductive structures is noted in Harré, *The Principles of Scientific Thinking*, page 10.

[37] Hoyningen-Huene, *Systematicity: The Nature of Science*, section 2.1.2.

[38] Cartier, "Mathemagics (A Tribute to L. Euler and R. Feynman)", sections 1 and 5. On how physicists might refrain from pursing certain lines of inference and thereby avoid some difficulties that lack of rigour might cause see Davey, "Is Mathematical Rigor Necessary in Physics?". There are mathematicians who argue that there is a place for a similarly liberal approach within their own discipline, although on the understanding that any lack of rigour should be signalled: Jaffe and Quinn, "'Theoretical Mathematics': Toward a Cultural Synthesis of Mathematics and Theoretical Physics". But there are also mathematicians who are concerned to emphasize the central role of proof: Mac Lane, "Despite Physicists, Proof is Essential in Mathematics".

[39] Meheus (ed.), *Inconsistency in Science*.

There are pervasive claims in physics, such as the laws of thermodynamics, but they tend not to limit options to the same extent that axioms shape mathematics. There are also sub-disciplines which are outliers relative to the central content of physics and which have a degree of independence from that central content, so that the concepts used and the consequent shapes of the sub-disciplines are not inevitable consequences of the central content. The study of friction, for example, developed over long periods before theoretical understanding came to be supplied.[40]

Finally, claims made within physics are often straightforwardly falsifiable.

When we move on to chemistry, we see more of the same type of development. Deductive relationships and relationships that are almost as strong matter, but large-scale deductive structures are not common. Pervasive claims are less dominant. New sub-disciplines are developed as the discipline advances, and their natures are not fully determined by the existing central content. These new sub-disciplines do not come from nowhere. They develop in relation to existing problems. They make extensive use of the existing corpus. And their development may feed back into that corpus, leading to fresh developments outside the new sub-disciplines. Even so, it seems that chemists do not find the natures of their sub-disciplines fully determined by

[40] Dowson, *History of Tribology*, gives a history of work done over millennia before discussing, in chapter 11, the theoretical understanding that developed in the twentieth century. It may be that in a discipline like physics, freedom to develop a sub-discipline independently of the central content of the discipline is only likely to be temporary, with a developing central content gradually coming to constrain the content of the sub-discipline. But at any given time there may well be examples like that of friction, developing with a degree of independence from the central content as it exists at that time.

the central content of the discipline as it exists at the time. The study of chemical bonds provides an example. That study progressed some distance before theories that would underpin it caught up.[41]

Despite the accentuation in chemistry of the developments that may first be observed in physics, chemists resemble physicists in preferring claims that are precisely quantified, a feature that helps to make claims straightforwardly falsifiable.

This course of development of the characteristics of disciplines continues into the life sciences.

1.5.3 The social sciences

The social sciences include the great bulk of sociology, economics and political science, along with some parts of psychology, geography, anthropology and history. Other parts of any of these disciplines may be regarded as humanities. Most parts of history are best regarded as humanities, while small and diminishing proportions of economics and psychology may be regarded as humanities. Yet other parts of some of these disciplines, especially parts of psychology, may be regarded as natural sciences. As with the natural sciences, the lack of a fully determinable extension for the concept of a social science will not matter.

The trends that we see as we work up the scale of the natural sciences continue into the social sciences. There are proportionately fewer deductive relationships between propositions or relationships that are almost as strong, and the extent to which pervasive claims tightly constrain the

[41] Gillespie and Popelier, *Chemical Bonding and Molecular Geometry: From Lewis to Electron Densities*, chapter 1.

making of other claims is less than in lower disciplines. In addition, claims that are straightforwardly falsifiable are proportionately less common than they are in the natural sciences.

Economics may serve as an example. Some claims are pervasive to a remarkable extent. For example, basic principles of utility and of the sort of decision-making one expects to see in markets are applied in the comparatively recent sub-discipline of family economics, even though a family is in many respects different from a market.[42] But there are areas in which the power of pervasive claims to constrain the making of other claims is limited. For example, long-established and highly-developed principles of microeconomics have so far been unable to adjudicate between Keynesian and New Classical approaches to macroeconomics.[43]

There are two further developments, the beginnings of which we may see in some of the natural sciences, which become very marked when we move into the social sciences.

The first development is that while there may still be plenty of deductive relationships between propositions and relationships that are almost as strong, relationships that are significantly weaker may form a significant and even a preponderant proportion of the total number of relationships that researchers bother to identify. Moreover,

[42] Becker, *A Treatise on the Family*, is a classic text. The adequacy of accounts of economic decisions within family life that are straightforwardly based on the economics of the market is however contested. See for example Folbre, *Valuing Children: Rethinking the Economics of the Family*.

[43] For an outline history of the debates and comments on the prospects for consensus on at least some issues see Snowdon and Vane, *Modern Macroeconomics: Its Origins, Development and Current State*, sections 1.7, 1.8 and 12.3.

the use of arguments that are constructed using these weaker relationships may be regarded as a perfectly good way to support claims, to an extent that would not be tolerated in physics or chemistry.

It has even been argued that use in the social sciences of deductive methods and methods of comparable rigour, such as statistical tests, can weaken research in those disciplines.[44] These arguments are however not general arguments against the use of all methods of that kind. They are arguments against the use of methods without taking account of relevant data, including data that would show the world to be more complicated than the methods tend to assume, and arguments against their use in ways that are insensitive to features of the world which must be discovered empirically rather than read off from the methods' presuppositions.

The second development is that a new vocabulary comes into use. This is the human vocabulary of motives, reasons and drives. It is radically different from the detached vocabulary in which the lower natural sciences are wholly conducted, and in which the higher natural sciences are mostly conducted. It is special for a number of reasons. Its terms lack the crisp definitions that would yield fully determinable extensions for the corresponding concepts. The vocabulary is the one that we use in everyday life, in understanding people around us and in managing our own lives. And the vocabulary encourages researchers to see goal-directedness. We shall refer to the way of speaking that uses this vocabulary as the human idiom, distinguishing it from the physical idiom that is generally used in the natural sciences. It does however include the physical idiom as a part of itself: the physical idiom, in the guise of a part of

[44] Kincaid, "Formal Rationality and Its Pernicious Effects on the Social Sciences".

the human idiom, is used extensively in the social sciences and the humanities. We shall explore the human idiom in section 2.2.2.

1.5.4 The humanities

The humanities study both relationships between people and relationships between the ideas that shape people's actions and interactions. Relationships between people are the dominant theme in most of social and political history. Relationships between ideas are the dominant theme in the history of ideas, in most of philosophy and in large parts of aesthetics and literary studies.

When we turn to those parts of jurisprudence and political science that are not better regarded as social sciences, we see how both themes can be equally important. A researcher who asks what would be the social effects of instituting certain legal or political relationships among people considers relationships between people. One who asks how legal and political ideas are related to one another and to other ideas, whether historically or independently of their history, considers relationships between ideas. In these and other disciplines, it may be impossible to state that one of the two themes predominates in a piece of work.

Despite such problematic examples, it is worth distinguishing the two themes because the theme of a piece of work will influence the extent to which it exhibits some characteristics that are significant for our purposes.

1.5.4.1 Features of the humanities

The features of disciplines that become conspicuous when we move up to the social sciences become even more so as we move into the humanities.

Deductive relationships between propositions and relationships that are almost as strong are proportionately less significant than in lower disciplines. But we must not think that they come close to vanishing, merely because it would be very unusual for the occurrence of one historical event to entail the occurrence of another. Deductive relationships and relationships that are almost as strong may still play roles not only in moving from specific pieces of evidence to verdicts on detailed claims, but also in the appraisal of claims that some events caused others or that given narratives appropriately represent courses of events. Roles for strong relationships that go beyond the immediate use of evidence have also been identified and discussed in relation to the social sciences, and particularly with reference to historical accounts that are given in the social sciences.[45]

We also find that pervasive claims impose only modest constraints on the making of other claims, and that claims are often not straightforwardly falsifiable.

The use of relationships between propositions that are significantly weaker than deductive relationships is perfectly respectable in the humanities. It is indeed essential to their practice. And the weakness of individual relationships is counterbalanced by the fact that many pieces of evidence can often be woven together to support claims.

[45] Mahoney, "The Logic of Process Tracing Tests in the Social Sciences".

When the focus is on relationships between people, the human idiom is particularly conspicuous. Researchers give accounts of people's actions in human terms. They identify influences on the people studied at least partly by paying attention to how those people perceived their own situations, and they see those people's choices in the terms in which we see our own choices.

When the focus is on relationships between ideas, the role of the human idiom as a whole, as distinct from those parts of it that are used to express the ideas in question, may not be so conspicuous. Instead, researchers work largely in terms of concepts in their own right, and in terms of the relationships between those concepts. That is likely to make deductive relationships between propositions, and relationships that are almost as strong, proportionately more significant than when the focus is on relationships between people. In philosophy for example, deductive arguments are often given, although ambiguities in the terms used and doubts about the premises mean that such arguments are not always secure against all reasonable challenges. And in the history of ideas researchers may see how one position gave rise to another through logical development, rather than only seeing psychological processes in the minds of the relevant writers.

This is not to say that the human idiom ceases to matter. Ideas such as those of perception, belief and political authority, as those ideas are used by philosophers and historians of ideas, would not make anywhere near enough sense to conduct the relevant disciplines without an understanding of human life in human terms. But to the extent that the human idiom plays only a supporting role of this nature, its significance may be less conspicuous than it would otherwise be.

We also do not deny the need to consider psychological processes, and the historical context within which ideas arose and were expressed or within which works of art were produced, whenever the study in question has a historical aspect. It would for example be foolish to detach the history of ideas from the story of the people who had the ideas and who were subject to contextual influences which were not exclusively intellectual. When researchers pay attention to the historical context in this way, the role of the human idiom may be just as conspicuous as it is in any other type of history.

Chapter 2

A Framework

2.1 Disciplines, topics, accounts

We shall see academic work as having a certain structure. There is a range of disciplines, each of which has its own norms and practices. Within each discipline there is a range of topics, some of which may overlap with or encompass others. Accounts say things about individual topics. Researchers may be happy to give a range of accounts of a topic, or they may seek a single best account. Within each account, a number of claims will be made. Some claims will come to be accepted.

Claims are not limited to claims within accounts. There are also claims that are made about accounts. Thus if researchers endorse an account, they will not only assent to the claims within it, or at least the significant ones. They will also assent to the claim that it is a good account. And if researchers consider one account to be superior to another one, they will assent to that claim too.

In the remainder of this section we shall say more about disciplines and topics, and then about accounts.

2.1.1 Disciplines and topics

The discipline very largely determines the concepts that are used, the types of topic that are identified, the types of evidence that are identified and the ways in which evidence may be used.

Within a discipline, researchers will choose topics to study. A physicist working within the discipline of superfluidity might study film flow or heat transport. An economist working within the discipline of optimal taxation might study the effects of different tax rates on total tax collected, or he might study the economic distortions created by different balances between taxes on income and taxes on consumption. And so on.

Not just any activity of giving accounts of topics qualifies as a discipline, not even when the topics are closely related and the accounts display a common approach to those topics. A discipline must have norms. Those norms must be good enough to steer researchers away from making or assenting to incorrect claims. If that standard is not met, we cannot regard the activity as a discipline.

2.1.1.1 Sub-disciplines

We shall sometimes want to consider sub-disciplines of large disciplines, rather than whole large disciplines. These sub-disciplines may be broad, like ecclesiastical history or molecular biology, or they may be narrow, like the

study of superfluidity or of optimal taxation. Even the narrow ones may have vast corpora and substantial research programmes, but they are still markedly narrower than other, broad, sub-disciplines.

A sub-discipline may contain sub-sub-disciplines. This will not however concern us. We shall use the term "discipline" to refer both to whole disciplines as commonly identified – physics, chemistry, economics, history and the like – and to sub-disciplines of any breadth. The term will refer to any coherent corpus and programme of research, however broad or narrow it may be. The term will therefore not be limited to traditional disciplines and sub-disciplines. It will extend to new disciplines and sub-disciplines, some of which either lie across the boundaries of established disciplines or draw on several established disciplines in order to define new areas of work.[1]

The division of disciplines into sub-disciplines is not arbitrary. Sub-disciplines are to be identified by reference to how researchers themselves think of their work. Reliance on how researchers identify sub-disciplines is important, because it allows us to think of the norms of each sub-discipline as established by reference to the practices of its researchers. If an indication of actual sub-disciplines

[1] For examples of boundary-crossing see Frodeman, Klein and Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*, chapters 6 to 9 (chapter 6: Crease, "Physical Sciences"; chapter 7: Calhoun and Rhoten, "Integrating the Social Sciences: Theoretical Knowledge, Methodological Tools, and Practical Applications"; chapter 8: Burggren, Chapman, Keller, Monticino and Torday, "Biological Sciences"; chapter 9: Klein and Parncutt, "Art and Music Research"). For examples of new disciplines that draw on ranges of established disciplines see chapters 15 and 16 (chapter 15: Briggle and Christians, "Media and Communication"; chapter 16: Thagard, "Cognitive Science" (a)). For a review of the proliferation of disciplines and sub-disciplines and of some effects on the process of research see chapter 1: Weingart, "A Short History of Knowledge Formations".

is needed, it may be obtained by consulting the titles of journals and conference proceedings.

Finally, it will not matter that the limits of sub-disciplines may be vague, or that sometimes a piece of work could be regarded as falling within either or both of two neighbouring sub-disciplines. Our arguments will not require us to possess a complete or precise taxonomy of areas of study.

2.1.1.2 The norms of disciplines

Disciplines are characterized by their norms, as well as by their subject matters and by the concepts that they define and use. The norms set standards of work. In so doing, they govern both the making of claims and the giving of assent. Claims should only be considered for assent if those who make them have done work in accordance with the norms. There are also norms that specifically govern the review of other researchers' work, and that therefore govern the process of consideration for assent. Norms can have a significant favourable effect on our confidence. We shall return to the role of norms in giving confidence in section 6.1.

In mathematics, norms of argument are precise: deduction is required. In the natural sciences, there is a shared understanding of how variables and hypotheses should be defined, how experiments should be designed and carried out, and how data should be analysed mathematically.[2] There is

[2] There is a substantial literature on how to design and conduct experiments and how to assess the results. Two examples are Ruxton and Colegrave, *Experimental Design for the Life Sciences*; Wu and Hamada, *Experiments: Planning, Analysis, and Optimization*. The term "experimental protocol" tends to be used for the rules that must be followed in order to complete an experiment correctly. But we

also a norm that experimental designs, descriptions of the conduct and the results of experiments, and descriptions of methods of analysis of results should be set out, so that researchers who consider results obtained by others can assess the work done. The fact that some have recently felt the need to set out standards of disclosure more clearly than hitherto suggests that standards are not always as high as they should be.[3] But on the positive side there are now several journals that specialize in setting out experimental procedures in detail, some of which publish videos online so that readers can watch researchers at work and notice details that may not have been written down.[4]

In the social sciences there are also standards which set out how data should be gathered and analysed. The norms will however be less precise than norms in the natural sciences, and there may be rather more scope than in the natural sciences to debate the norms. Lack of precision and scope to debate norms will both reduce the extent of the favourable effect on our confidence. There are plenty of texts that give researchers guidance on how studies should be designed and conducted and on pitfalls to avoid, but no one text can be

shall refer to the conduct of experiments because our concern is with what happens, rather than with what should happen, even though the distinction will often not matter because results are not usually reported until experiments have been completed in accordance with their protocols. This will allow us to refer to the designs of experiments separately, with no risk of confusion between designs and either desired conduct or actual conduct. It is worthwhile to avoid confusion in this area: Sullivan, "The Multiplicity of Experimental Protocols: A Challenge to Reductionist and Non-Reductionist Models of the Unity of Neuroscience", section 2.

[3] A review of papers in scientific research journals will show how the necessary information is set out. For work on making standards clearer see Nosek et al., "Promoting an Open Research Culture".

[4] Examples include the *Journal of Visualized Experiments*, http://www.jove.com; *Current Protocols*, http://www.currentprotocols.com; *Nature Protocols*, http://www.nature.com/nprot/.

regarded as definitive.[5]

Moving on to the humanities, the norms are even less precise, and are largely implied by practice rather than set out in manuals. It does not follow that they are more open to debate than norms in the social sciences, because lack of precision and lack of explicit statement can make it hard for a debate to get started. But the lack of precision and lack of explicit statement themselves reduce the extent of the favourable effect on our confidence.

Within the humanities and the social sciences, there are norms that relate directly to the handling of evidence and norms that relate to the interpretation of evidence.

Norms that relate directly to the handling of evidence may be open to precise formulation, and this may work against the tendency to decreasing average precision. For example, there are some precisely specified methods to be used in archaeological excavations.[6] Methods are not norms in themselves, but when methods are specified there is an associated norm that they should be used. To take

[5] Some examples of texts are Matthews and Ross, *Research Methods: A Practical Guide for the Social Sciences*; Yin, *Qualitative Research from Start to Finish*; Miles, Huberman and Saldaña, *Qualitative Data Analysis: A Methods Sourcebook*; Miller and Salkind, *Handbook of Research Design and Social Measurement*; Bernard, *Research Methods in Anthropology: Qualitative and Quantitative Approaches*; Friedman and Sunder, *Experimental Methods: A Primer for Economists*. For a brisk survey of debates over method in the social sciences, with ample references to literature, see Poteete, Janssen and Ostrom, *Working Together: Collective Action, the Commons, and Multiple Methods in Practice*, pages 3-11. Examples of texts on psychology, some parts of which fall within the natural sciences and some within the social sciences, include Shaughnessy, Zechmeister and Zechmeister, *Research Methods in Psychology*; Little (ed.), *The Oxford Handbook of Quantitative Methods* (two volumes).

[6] Roskams, *Excavation*.

another example of a norm, one which needs no technical specification of method to convey the substance of what it prescribes and which is so obvious that it is unlikely to be made explicit at all, a political historian should look at the surviving private correspondence of leading figures as well as looking at official documents.

Norms that relate to the interpretation of evidence tend to be general guides rather than precise rules. For example, there is a norm that historians should avoid presentism, that is, various forms of reading their own perspective back into the past.[7] Another example of a norm of interpretation is that claims in the philosophy of mind should be checked for compatibility with the latest findings of cognitive science and neuroscience.[8] There is also reason to think that the social sciences generally, and social and cultural anthropology in particular, could benefit from making more use than they have hitherto made of work in cognitive science.[9] There might in due course arise a norm that social scientists should review their interpretations for compatibility with the findings of cognitive science, and for

[7] Hunt, "Against Presentism", decries the vice. Fischer, *Historians' Fallacies: Toward a Logic of Historical Thought*, pages 135-140, analyses it.

[8] This is not to propose that these sciences and philosophy of mind should be related as masters and slave. But even those who maintain that neuroscience is in urgent need of philosophical advice, such as Bennett and Hacker in *Philosophical Foundations of Neuroscience*, do not suggest that philosophers of mind should disregard neuroscience. Reassuringly, there is evidence from papers in journals that philosophers of mind have in recent decades engaged more and more with cognitive science (Knobe, "Philosophers Are Doing Something Different Now: Quantitative Data"), although this must in part reflect the fact that both the quantity and the quality of cognitive science research have increased markedly over the relevant period.

[9] Bloch, *Anthropology and the Cognitive Challenge*, particularly chapters 5 to 8.

opportunities to enrich their interpretations by reference to those findings.

Norms are by and large easily distinguished from factual claims. Norms tell researchers how to work, while factual claims tell them about the world. But specific norms of method and factual claims are intertwined in two ways.

The first way is this. The nature of the world determines which methods are most likely to lead researchers to make claims that are correct. The nature of the world thereby implies norms that certain methods should be used. The usefulness of methods is underwritten partly by facts within the scope of the discipline in question, and partly by more general facts. For example, the usefulness of a way to detect neuronal activity will depend partly on facts about neurons that are specific to neuroscience, and partly on some more general facts of physics and chemistry that will affect how detectors work and what may go wrong.[10] One very general fact, standing in the background in relation to all of the natural sciences and many aspects of other disciplines, is that the world is apt to be modelled by the mathematics that we use. This fact supports the widespread norm that researchers must make statistical tests of the significance of the results of experiments.

The second way in which norms and factual claims are intertwined is a limiting case of the first way. There are factual claims made within disciplines that directly imply norms. For example, historians may claim that in a particular war governments fed false information to journalists as well as correct information, in order to keep up morale or deceive the enemy. That would imply a norm

[10] A survey of one range of techniques that illustrates the relevance of a wide range of different facts is Peterka, Takahashi and Yuste, "Imaging Voltage in Neurons".

for historians to the effect that newspaper reports from the period should be regarded with great suspicion.

2.1.2 Accounts

Having gathered and studied evidence, researchers may give some account of a topic. An account is a set of claims, unified by their concern with some single topic and by relationships to one another which structure the account.

An account need not be comprehensive. Comprehensive accounts are mainly found in textbooks, which only give secondhand reports of research. Research papers sometimes offer only fragmentary answers to the questions that researchers ask, and include proposals for further work to fill in the gaps. (This may be seen from a review of the discussion sections of many papers in the natural sciences.) But there is still a unity given by the subject matter and the work done. And when an account offers an explanation of some phenomenon, rather than merely offering potential components of an explanation, it makes perfect sense to appraise the account as a whole, and not merely the claims within it. (We shall say something about the endorsement of explanations in section 5.1.2.)

Accounts may be more or less closely tied to specific occasions. At one extreme, accounts in history are largely reports of what happened at particular times and places, and are not expected to have any application to other times or places. At the other extreme, accounts in physics and chemistry mostly set out what is expected to happen at all times and places. Some theoretical accounts in economics and sociology fall between these two extremes, setting out what would be expected to happen in situations of certain broad and perhaps imperfectly specified types. Different

types of account may be used together, as when one account sets out some general claims and a second account applies those claims to a specific time and place. And some accounts, such as those in pure mathematics, are not about the spatio-temporal at all.

2.1.2.1 Complementary and conflicting accounts

There may be just one account of each topic. But in all disciplines there can be several different accounts of a single topic, with the accounts being complementary rather than conflicting with one another. (We shall take complementarity to arise when accounts cover the same ground in different ways, rather than being fragmentary accounts that cover non-overlapping parts of their topic.) Researchers may keep a range of accounts in play for various reasons. Some accounts may be better than others at helping researchers to think about topics and tackle outstanding questions. Some may be more computationally convenient than others. And sometimes researchers take the view that no one account could ever be wholly satisfactory, so that an ensemble of accounts will always be needed. But there is an important difference between disciplines. At the low end of the scale of disciplines, there may be scope to regard a single account as comprehensive. Then other accounts might be kept in play merely for heuristic or computational reasons. Higher up the scale it is more likely that several complementary accounts would always be needed, because it would not be possible to regard any one account as comprehensive.

Mathematics is a special case. There may be several derivations of a result. The derivations are explanatory accounts in that they show why the result arises. Mathematicians may prefer some derivations to others because they give

a better idea of the reasons why it arises. But the world of mathematics is independent of the physical world, and all parts of the world of mathematics, including all of the deductive relationships between propositions, can be regarded as equally real. This makes it difficult to say that some derivations represent the world of mathematics better than others.

In physics and chemistry, it is easy to think that it would be possible to find a single comprehensive account of a topic. The precision of concepts and the limited range of things to say about objects of study might make this possible.

Then there would be no pressure to have a range of accounts of the same topic, and indeed some pressure to select the most comprehensive available account. If several accounts are in use even though a comprehensive account that seems to represent the word very well has already been found, it may be simply because some of them have heuristic value, or offer computational convenience while giving approximate answers that are good enough for many purposes. For example, an account of chemical bonds within molecules can only be considered to represent the world properly if it takes full account of electron correlations. But that requires use of the full configuration interaction method, which is computationally demanding. Other approaches are therefore often used.[11]

In natural sciences that are comparatively high up the scale one can argue for the need for overlapping explanations at different levels in order to give an adequate representation of the world, even when accounts already given are by no

[11] Details can be found in Piela, *Ideas of Quantum Chemistry*, chapters 8 to 11. The alternative approaches are by no means all unsatisfactory, nor are they all ones that have ceased to be developed. Density functional theory, which Piela covers in chapter 11, is both successful and undergoing continued development.

means fragmentary. An example is given by the homeostasis of the human body. It falls within the scope of, and can be explained in terms of, both biochemistry and systems theory.[12] Both types of explanation are valuable, and a full picture is only given when they are both provided. But the two types of explanation remain distinct. Another example is supplied by the study of human mental processes.[13] Moreover, the levels themselves need not be seen as arbitrary. There is scope to identify levels worth investigating as "local maxima of regularity and predictability", a method that saves identifications from being artefacts of the immediately relevant theories. (They may however be artefacts of broader theories of what constitutes organization.) But the scope to use this method to identify levels worth investigating diminishes as we go up the scale of disciplines.[14]

In the social sciences and the humanities, researchers are even more likely to conclude that two or more accounts of the same topic that are far from fragmentary should all

[12] Buchman, "The Community of the Self".

[13] Craver, *Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience*, chapter 1, sections 3 and 4, and chapter 7. Craver maintains the need for explanations at different levels alongside, and not in contradiction to, a view that neuroscience searches for mechanical explanations. But he does not see a plurality of explanations as ultimate. Rather, he sees explanations at different levels as coming together to form a single overall explanation, albeit without erasing the contributing explanations or hiding their different levels from view (chapter 7, section 4). For criticism of Craver's argument see Sullivan, "The Multiplicity of Experimental Protocols: A Challenge to Reductionist and Non-Reductionist Models of the Unity of Neuroscience", section 4; Johnson, "The Relationship Between Psychological Capacities and Neurobiological Activities", section 2.5.

[14] Wimsatt, *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*. See pages 209-211 for the possibility (the phrase "local maxima of regularity and predictability" is on page 209). See pages 227-240 for limits to the approach.

be kept in play, not merely for heuristic or computational reasons but also, and more importantly, because the world is too complicated, and concepts in use are too imprecise, to allow any one account to be comprehensive. Several explanations of a given phenomenon may all need to be viewed together. To take an example from economics, there are several different ways to explain economic growth which place varying degrees of emphasis on factors such as physical capital, population changes, technology, government action and culture, and which use different mathematical models of growth.[15] Another example, taken from history, is the identification of economic and social facts, and also facts about political structures, power and ideas, in order to provide contexts within which the European revolutions of 1848 make sense.[16] A third example is given by the fact that explanations of the Reformation in England can focus on doctrine or on high politics. Explanations from different perspectives can fit together to paint a fuller picture.[17]

Examples like these can raise a terminological point. Depending on how a topic is defined, accounts might be seen as accounts of different topics rather than different accounts of the same topic. But there are perfectly acceptable ways to identify topics which will lead us to see different accounts of the same topics. In these examples we may define the topics exactly as we have done: as homeostasis of the human body, human mental processes, economic growth, the revolutions of 1848 and the Reformation in England.

Although the goal remains that of representing the world, there is an important point of contrast with different

[15] Weil, *Economic Growth*; Aghion and Howitt, *The Economics of Growth*.

[16] Sperber, *The European Revolutions, 1848-1851*, chapter 1, chapter 2 and pages 109-116 and 258-264.

[17] Marshall, *Reformation England 1480-1642*, pages 37-38.

comprehensive accounts of single topics that co-exist in physics and chemistry. We should not see co-existing accounts in the social sciences and the humanities as approximations to an ideal theory. There is no such ideal theory to be had, at least not in the foreseeable future. It would even be dangerous to see them as presenting different facets of some fully integrated single account that fell short of being a theory. To return to the example of economic growth, even the approach that goes under the name of unified growth theory does not ground all of the factors that promote economic growth in some single underlying reality that is specified in any detail. Instead it offers one way to combine the influences of the separate factors, argues for some relationships between those influences, and identifies triggers for growth.[18]

While researchers in higher disciplines may often make room for several accounts of the same topic, their commitment to the pursuit of correctness requires them not to rest content when accounts conflict with one another. A conflict between accounts creates pressure to resolve that conflict, even if researchers cannot currently see how to resolve it. Even if the notion of faultless disagreement is a coherent one, it cannot, in the context of disciplines that seek to find out about the world, carry the implication that more than one party to a disagreement is correct.[19]

[18] Galor, *Unified Growth Theory*. Galor notes at the start of section 5.3 that several factors interact. We should also note that unified growth theory does not cover all variations in economic growth. It concerns long-term phenomena, and in particular the transition from a long period of relative stagnation to remarkable growth in the last two centuries, rather than variations in growth rates over periods of a few years.

[19] On difficulties for the notion of faultless disagreement, although in the context of everyday claims rather than in the context of academic disciplines, see Buekens, "Faultless Disagreement, Assertions and the Affective-Expressive Dimension of Judgments of Taste". For

In section 5.4.3.2, we shall return to the existence of ranges of accounts. There we shall consider the significance of the existence of different explanations of phenomena for our confidence.

2.2 Idioms

One important feature of a discipline is the idiom in which it is conducted. We shall identify the physical idiom and the human idiom.

The physical idiom is the idiom of the natural sciences. It is distinguished by its independence from specifically human ways of conceiving the world. Language that expresses how things seem to human beings, or that echoes the way in which it seems to human beings that they deliberate before action, is absent from nearly all accounts in the natural sciences, apart from accounts in psychology. On the other hand, such language is essential to the humanities and to a wide range of work in the social sciences. We shall refer to the idiom that uses such language as the human idiom.

We shall first describe the physical idiom, in order to give a background against which some contrasting features of the human idiom will stand out. We shall then describe the human idiom.

an argument that in some circumstances sustained disagreement does not show irrationality see Kelp and Douven, "Sustaining a Rational Disagreement".

2.2.1 The physical idiom

Terms that are used in the natural sciences cover an enormous range, from “electron” in physics, through “hydrogenation” in chemistry, up to “food web” in ecology. The terms may be more or less precisely defined, and the effects of use of the corresponding concepts on the amount of microphysical detail of the world that is captured or overlooked vary hugely. It is essential that there should be such a wide range of concepts available. Researchers in the different natural sciences need to have the conceptual equipment to take steps forward. In particular, they need to be able to overlook enough microphysical detail of the world to give a manageable picture at the level of the discipline concerned, while still identifying salient features of the world so they can give accounts that are coherent and explanatory. The higher up the scale of disciplines one goes, the more complicated are the objects of study and the more detail must be overlooked. The wide range of terms in use makes it appropriate to speak of the many overlapping vocabularies of the physical idiom, rather than a single vocabulary.

We may ask why we should group all of these different vocabularies together under the heading of the physical idiom. One reason might be the existence of scope to reduce concepts used at higher levels on the scale of disciplines to complex assemblies of concepts used at the level of physics. Such a reduction would involve elaborate definitions of terms used at higher levels, where the definitions were given in the terms of physics. But it is not at all clear that such a reduction would be possible. It is certainly not widely practised. We need some other reason to group the different vocabularies together.

We propose the fact that accounts given in the physical idiom would be fully intelligible to a wide range of rational beings with enough intellectual power. (We shall take the qualification about intellectual power as read from now on.) The rational beings to consider are not those who happen to exist. We have no idea what rational beings exist apart from ourselves. We are to consider the rational beings we can imagine.

The wide full intelligibility of accounts that are given in the physical idiom can be expressed as its not being necessary to appreciate the human way of looking at the world in order to make sense of those accounts. Aliens should get practically as much out of accounts as human beings would get out of them. There is no non-trivial content of an account that would only be available to human beings and to a narrow range of other beings who were very like human beings.

The notion of a wide range of rational beings is vague, but we can give it some substance by contrasting it with the notion of a narrow range. We can imagine a narrow range of rational beings who would be quite like us in that they would have needs, desires, motives and habits of thought that were analogous to our own. They might make sense of work done by human beings in the social sciences and the humanities, work in the human idiom. Then there would be rational beings in a wider range who lacked such features and who could only make sense of work done by human beings in the natural sciences, work in the physical idiom.

We shall not be so rash as to make any claim about all rational beings, or even about a majority of rational beings. There might well be rational beings who shared so few of our ways of thinking about the world that even our physics would not be fully intelligible to them, or who had started from a position in which they could have understood

our natural sciences, but who had since developed their sciences in such a different direction that they would no longer be able to understand. All we need in order for our criterion to be usable is that the range of rational beings who could grasp accounts in the physical idiom should be a good deal wider than the range who could grasp accounts in the human idiom. We need a marked step down from a wide range to a narrow range in order to distinguish accounts in the two idioms, without being left with an unacceptably large number of accounts that the criterion could not identify as being in one idiom rather than the other. We can find such a marked step down without any need to consider the full range of beings with radically different modes of thought that Nicholas Rescher contemplates.[20] And we should also be able to find a marked step down even if the ranges of rational beings who would find our chemistry and biology intelligible were narrower than the range who would find our physics intelligible. The step down from the natural sciences to the social sciences and the humanities should still be much more marked than the step down from physics to biology.

We can appreciate the effect of the criterion of wide full intelligibility by considering disciplines within which the criterion is not satisfied. These are disciplines that draw on specifically human ways of interacting with the world, disciplines within which we rely on our way of life and on the deliverances of our particular senses in order to make sense of things. Consider for example a historical account. Explanations of why people acted in certain ways make sense to us because we too act in certain ways for specifically human reasons. Our actions reflect both what motivates us, and our propensities to be particularly aware of features of the world of certain types. We can also see the importance of

[20] Rescher, *Unknowability: An Inquiry into the Limits of Knowledge*, chapter 3.

the human way of life in much of economics. The discussions of desires and their satisfaction that underpin discussions of utility, of the shapes of demand curves and of the allocation of resources make sense to human beings because human beings have desires and seek to satisfy them. There is no reason to think that a wide range of rational beings would act from what human beings might recognize as motives, would be particularly aware of features of the world that human beings found significant, or would have desires in the sense that human beings had them. We could not expect more than a narrow range of rational beings to get the point of accounts within the disciplines of history or economics.

In disciplines that limit themselves to the physical idiom, it should be possible to find wide ranges of rational beings who would not suffer a loss of content when they considered accounts, although as already noted, the maximum width of ranges might be less for chemistry and biology than for physics. While many different types of rational being might respond to the physical world differently from human beings by virtue of their physiologies, they should all be able to consider the physical world independently of how they would react to it and independently of how their own perceptual apparatus presented it to them.

To give a few examples, electrons, molecules of various sorts, temperatures and the structures of atmospheric circulation can all be investigated using instruments that could provide outputs in forms to suit a wide range of types of sensory and cerebral apparatus. The instruments might be similar in the laboratories and field stations of many different types of rational being. Indeed they might need to be similar, on account of the nature of the physical world. That would not limit the scope to present outputs in different ways. Moreover, forms of presentation by instruments would not irremediably infect accounts that were given. Accounts

might reflect forms of presentation, but the information that was contained in measurements taken and in analyses of data could be extracted and put in forms that were intelligible to other rational beings who would have required different forms of presentation.

We do here leave out of account the risk that different sensory and cerebral apparatus might lead other rational beings to have radically different conceptual schemes. It might do so, but as already noted we do not need to consider all rational beings. We may safely omit many of them from consideration and still establish a marked step down in the range of rational beings who could grasp accounts that were given in the human idiom. We also put to one side the question of the extent to which the use of instruments makes observations dependent on the theory that explains how the instruments work, as well as putting aside the question of whether there are other reasons why it would not be proper to think of the same observations as available to beings with different theories. Such questions are important, but we would have to range far beyond the bounds of our enquiry to address them. And in any case, we do not need observations to be totally independent of theory.

Certain concepts would need to be shared by rational beings in order to allow them to understand accounts given in the physical idiom, but these concepts would not be tied to specific varieties of apparatus of perception and thought, so they should be shareable across a wide range of rational beings. The necessary common ground might include the concept of spacetime (considered as a mathematical structure with a metric rather than a box within which objects were laid out together with a ticking clock), the concept of separate objects that were demarcated as the occupants of regions within spacetime, the concept of change, some notion of the significance of

proximity or distance in spacetime as affecting what might be taken into account in explanations of given changes, and concepts such as those of energy and entropy.

The need for a concept of causation, such as may be acquired from the everyday experience of acting and thereby making some difference in the world, would be more debatable. There are those who would expect it to be needed.[21] We could expect a wide range of rational beings to have the experience of acting and thereby affecting the world, although we could not be at all confident that they would have a phenomenology of action that was like our own. There is a risk that the phenomenology would both be important and be limited to a narrow range of beings, so that accounts in the physical idiom would not in fact be widely intelligible. But this risk is small enough that we need not be greatly concerned that it would render our criterion of wide intelligibility unusable. And the marked step down that we need could be available even if we had to omit many rational beings from the wide range because of their lack of appropriate phenomenology.

We may take the same attitude to the possibility that a substantial range of contributions of human minds to human experience might be necessary in order for accounts in the physical idiom to be intelligible. Such a range might be rather like the range that Immanuel Kant identified, although it might well differ from Kant's range in detail.[22]

[21] Heidelberger, "Causal and Symbolic Understanding in Historical Epistemology", and in particular his reference on page 476 to "our common causal intuitions". Heidelberger also sees a concept of causation as playing a vital role in an understanding of scientific instruments (page 477). To that extent, a concept of causation might be needed to underpin the acceptance by rational beings of the use of scientific instruments to obtain data and to present data in appropriate forms.

[22] For the alleged necessity of the categories to the experience

It is entirely possible that alien minds could make the same range of contributions. Moreover, we may disregard the risk that the ability to make those contributions would depend on sharing human phenomenology. We have no reason to think that this risk would be large, and in any case the need for shared phenomenology would not necessarily prevent identification of the necessary marked step down, especially not if the requirement was to share only the structure of human phenomenology, without the content that is allegedly given by qualia.

There would also need to be a shared understanding of the project of the explanation of phenomena. And at a more basic level, accounts given by human beings in the physical idiom might only be intelligible to other beings who shared the alleged intentional nature of human observation of the world as a form of action, the action of observing an object which is for that purpose brought under some particular description, rather than observation's being the passive absorption of stimuli.[23] These requirements should however be met by a wide range of beings.

The requirement of wide full intelligibility is not a requirement that there should be some absolute conception of the world, whether a conception that would somehow underpin representations of the world in other conceptual schemes or a conception on which rational beings of many types would converge. It is not even a requirement that a wide range of rational beings should agree on the best accounts to give. It is merely a requirement that a wide range of rational beings should be able to understand the accounts

of objects see Kant, *Kritik der reinen Vernunft* (*Critique of Pure Reason*), A93/B126.

[23] An argument for the intentional nature of observation is given in Buekens and Muller, "Intentionality versus Constructive Empiricism", section 1.

that human beings gave in the physical idiom, in order for us to judge that those accounts were in that idiom.[24]

We should give special consideration to mathematics, which is both a discipline in itself and the source of much of the vocabulary and many of the methods of the natural sciences and of some other disciplines. There are grounds to think that mathematics would be intelligible to a very wide range of rational beings. The discipline consists in the manipulation of symbols that represent abstract structures and their features. The structures need not have anything to do with any particular form of life. And the idea of mathematics tailored to the mentality of a particular species, as opposed to mathematics that would be intelligible to a wide range of rational beings, would not make sense.[25]

[24] We may compare Bernard Williams's thoughts on an absolute conception of the world: Williams, "Philosophy as a Humanistic Discipline", section 3. On the one hand, there is a point in common. Williams writes of "a kind of representation that might be reached by any competent investigators of the world" (page 185). He does not require non-human beings actually to reach any given representation. And if other rational beings could grasp accounts that human beings gave in the physical idiom, then it is probable that they would also have been able to reach those accounts. On the other hand, there is a point of contrast. Williams writes of an absolute conception as if it were a conception that had no special connection with any particular kind of rational being, rather than starting from the conceptions that human beings have and asking whether other beings could grasp them.

[25] We are untroubled by the argument of Lakoff and Núñez, *Where Mathematics Comes From: How the Embodied Mind Brings Mathematics into Being*, that our mathematics depends on our experience. The arguments of that book do not exclude the possibility of beings with quite different experience having access to the same mathematics, nor do they exclude the possibility that all rational beings would converge on the same mathematics. (Convergence means that they would converge on the same large collection of structures and theories. They would, for example, all come to have both Euclidean and non-Euclidean geometries in their repertoires. It does not mean that they would all come to agree that some particular geometry was

We shall treat mathematical expressions as expressions in the physical idiom when they are used either to conduct mathematics as a discipline in itself, or to conduct other disciplines that are themselves conducted in the physical idiom. We must also allow for the use of mathematical expressions in disciplines that are conducted in the human idiom. In that case the mathematical expressions might not have, to non-human rational beings, the full significance that they would have to human beings, even though they would still have some significance simply as pieces of mathematics.

2.2.2 The human idiom

2.2.2.1 Psycho-social understandings

The human idiom is the idiom of the intentional stance, the stance that we adopt when we think of ourselves and others as sensitive, reflective and autonomous beings. It is the stance that we use both in conducting our lives and in understanding ourselves and other people.[26]

Although the intentional stance is a tool for living, our focus is on understanding rather than on practical life. The type of understanding of conduct that will concern us here is based on possession of what we shall call a

the right one.) Indeed, the authors reject radical cultural relativism (page 362). We may also note that the authors' claims for the centrality of metaphors have been challenged, for example in Madden, "Book Review: Where Mathematics Comes From: How the Embodied Mind Brings Mathematics into Being".

[26] We take the general notion of the intentional stance from Dennett, *The Intentional Stance*, but we shall not concern ourselves with detailed refinements. Nor shall we be concerned with debates about the reality of mental states or the importance of their reality.

psycho-social understanding. This is an understanding that people have of people and social environments in general, an understanding of the type that they exhibit in everyday life when they adopt the intentional stance. It will include a grasp of character traits, motives, emotions and what it is like to make decisions, along with an understanding of how people located in groups think, feel and conduct themselves.

Psycho-social understandings are not only used in everyday life. They are also used by researchers in the social sciences and the humanities in order to explain human conduct. Researchers may modify everyday psycho-social understandings, but modified versions remain reasonably close to everyday versions. The human idiom is the idiom that is used both to express the contents of psycho-social understandings, and to put such understandings to work in explaining human conduct.

There are two terminological points to note. The first point is that the indefinite article in "a psycho-social understanding" is important. Different people may have different understandings, and people may modify their own understandings. (We shall however speak of a single human idiom, meaning the union of all idioms that are used when making use of the various psycho-social understandings. Psycho-social understandings have enough in common to mean that both the ranges of terms in the corresponding idioms and the meanings of terms do not vary so greatly as to make this unification unreasonable.) The second point is that we choose the term "psycho-social understanding" in order to recognize that such an understanding relies not only on a grasp of the psychology of separate individuals, but also on a grasp of society. We do not intend to refer to any similarly named school of psychology.

Content and use

A psycho-social understanding comprises concepts and principles that allow people to explain the conduct of individuals and groups, and sometimes to predict that conduct. The understanding is used to pick out characteristics, habits of thought, drives, motives, intentions, propensities to respond to social contexts in certain ways, and the like. Principles that are contained within the understanding then make it possible to give explanations which relate the conduct observed to features that are picked out. The identifications and explanations are ones that most people from the relevant society would regard as sensible.

One might for example rely on the principle that ambition sometimes leads to the flattery of superiors and the undermining of rivals in order to explain such conduct on the part of an individual who had also shown other evidence of being ambitious. Or one might explain the continuing loyalty of sports fans to a team that was going through a bad patch by reference to connections between loyalty and the sense of identity of individuals.[27]

A psycho-social understanding is founded on folk psychology. This is an everyday grasp of the character traits that people exhibit, the influences under which they act and their consequent likely responses to different situations. Folk psychology includes not only specific expectations but also human beings' representations of themselves and other people as self-directed beings who make decisions freely, and who then act on those decisions unless they are diverted from doing so by other considerations or by weakness of the will. But we speak of psycho-social understandings rather than simply of folk psychology for two reasons. The first reason is that we want to allow for

[27] Connor, *The Sociology of Loyalty*, pages 108-109.

refined psycho-social understandings that researchers base on everyday understandings. Such refined understandings might be too refined to be in common use among the folk. The second reason is that we want a constant reminder that an understanding of society matters, just as much as an understanding of individuals.

Our focus on understanding rather than on practical life means that we shall generally think in terms of an articulable, if sometimes naive and platitudinous, theory of human motivation and conduct, rather than in terms of a skill that is exercised by simulating the mental processes of others or by constructing and using narratives. But this is not a momentous preference. The differences between the approaches matter a great deal in some areas of philosophy, but they need not trouble us.[28]

It would be tempting to use the term "folk sociology", but we shall resist. The term is even less well-defined than the term "folk psychology". Moreover, a good deal of the serious work that has been done so far in relation to folk

[28] For an account of the use of an articulable theory in understanding others see Malle, *How the Mind Explains Behavior: Folk Explanations, Meaning, and Social Interaction*. Malle delineates the content of a folk theory quite narrowly and precisely in section 2.1. We shall not be so narrow or precise in relation to psycho-social understandings. We shall include everything from the basic principles that other people have minds and intentions, right up to detailed expectations of human conduct that may be specific to individual societies. But such generous additions to Malle's notion would not prevent us from fitting our view of the use of a psycho-social understanding into the framework that is provided by his account of the use of folk theories in understanding others, even though his restriction of the content of a folk theory is important to the course of his own argument.

For simulationism see Goldman, *Simulating Minds: The Philosophy, Psychology, and Neuroscience of Mindreading*. For the roles of narratives see Hutto, *Folk Psychological Narratives: The Sociocultural Basis of Understanding Reasons*.

sociology has been concerned with the development of social awareness in children, rather than with defining the content of a shared adult understanding.[29]

Any use of the human idiom, whether in everyday life or in academic research, will reflect some psycho-social understanding or other. Academic researchers may use a psycho-social understanding that is more refined than understandings used in everyday life. Researchers may for example use a sophisticated concept of psychological stress which allows them to make connections with a number of other aspects of human life that are themselves characterized in reasonably technical terms.[30] But the process remains one of refinement within the broad project of understanding human beings as self-directed beings who are not radically different from the researchers, and of giving accounts that are accessible to those who rely only on the psycho-social understandings they use in everyday life. To take the example just cited, the concept of stress that is used is recognizable as a refinement of everyday concepts of stress. As a further illustration, we may note that analytical sociologists can do a great deal with an everyday understanding of people that is only formalized to a limited extent.[31]

[29] For the development of social awareness in children see Banaji and Gelman (eds.), *Navigating the Social World: What Infants, Children, and Other Species Can Teach Us*. For an example of work on the development in children of the tendency to place people in groups see Hirschfeld, "On a Folk Theory of Society: Children, Evolution, and Mental Representations of Social Groups".

[30] Wiklund, Öhman, Bengs and Malmgren-Olsson, "Living Close to the Edge: Embodied Dimensions of Distress During Emerging Adulthood", pages 2-3.

[31] See for example the use that is made of the DBO (desires-beliefs-opportunities) model in Hedström, *Dissecting the Social: On the Principles of Analytical Sociology*, chapters 3 and 4. Hedström and Bearman (eds.), *The Oxford Handbook of Analytical Sociology*, chapters 3 to 10 show that the categories used by analytical

We have so far taken it for granted that many explanations, in many disciplines, make use of everyday psycho-social understandings. We take this to be manifest from published work. It would be philosophically controversial to claim that this was how the relevant disciplines should be conducted. But we do not need to make that claim, because we are concerned with how disciplines are in fact conducted and with effects on our confidence. We may also note that at least some of the controversy would reflect concerns about the appropriateness of regarding folk psychology as a tool of theoretical understanding in practical life. Such concerns may be expressed in general opposition to theory theory, or they may take the form of a specific claim that in everyday life people use folk psychology to obtain information that is of practical use, rather than to map out people's minds.[32] But these concerns should not extend to the use of psycho-social understandings in academic disciplines, because it is standard to conduct disciplines in a detached way and to make many things explicit.

Extension to an understanding of social entities

It is possible to study human society either from a position of methodological individualism, or from a position of methodological collectivism (also known as methodological

sociologists, categories such as those of emotions, beliefs, preferences and norms, are perfectly everyday ones, although they may be made a little more technical than everyday life would demand. For an argument that sociology is unlikely to need a general theory of action see Hedström and Ylikoski, "Analytical Sociology and Rational-Choice Theory", sections 2.4.4, 2.6 and 2.7. Such a general theory might not be expected to correspond closely to an everyday understanding of human conduct, so if it were not needed, that would at least suggest the adequacy of an understanding which was not very different from an everyday one.

[32] Such a claim is made in Bogdan, "Common Sense Naturalized: The Practical Stance".

holism). Neither position is defined at all precisely but broadly, individualists think it appropriate to regard institutions, practices and other social entities as dependent for their existence and their behaviour on the thoughts and conduct of individuals, while collectivists think that in order to understand the human world we must be willing to see such entities as leading lives of their own, potentially to the point where they could be seen as steering history and pulling human beings along with them.[33] There is also scope for intermediate positions, for example a view that there is causation at the social level while agency remains at the level of individuals.[34]

To the extent that researchers considered methodological individualism to be an adequate approach, the characterization of psycho-social understandings that we have given would suffice. But to the extent that they considered methodological collectivism to be required, more would need to be added. It would not be possible to derive a sufficiently informative set of principles of the behaviour of social entities from principles that governed the conduct of individuals and of groups regarded as assemblies of individuals. (If that were possible, methodological collectivism would not be required, although it might happen to be practised.) The required additional principles of the behaviour of social entities would not be based on everyday psycho-social understandings. An expanded psycho-social understanding that researchers used might to that extent strike non-researchers as odd.

We shall not pursue the point here. But we shall note that if methodological collectivism were required, the connection

[33] For a survey of the debate see the papers in Zahle and Collin (eds.), *Rethinking the Individualism/Holism Debate: Essays in the Philosophy of Social Science*.

[34] Tuomela, "Holistic Social Causation and Explanation".

between psycho-social understandings used in research and their everyday equivalents would be weakened.

There are two other options to consider. The first option is methodological localism. This recognizes the reality of the social, but also sees that reality as embodied in the lives and outlooks of individuals.[35] Methodological localism should preserve a reasonably strong connection between psycho-social understandings used in research and their everyday counterparts, because of the central role that it gives to the natures of individuals. The second option is structural individualism. This emphasizes the importance of the structures that relate individuals, but it still takes individuals and their actions to be primary, and it also allows the use of everyday categories when describing influences on individuals. Structural individualism is particularly associated with analytical sociology.[36] As we noted above, the categories that analytical sociologists use to set out the significant influences on individuals, categories such as those of emotions, beliefs, preferences and norms, are also central to everyday psycho-social understandings. This indicates that the use of structural individualism should not require any great distancing from those everyday understandings.

[35] Little, "Levels of the Social". Little introduces the position of methodological localism on page 346, and "the socially constituted person" on pages 351-352.

[36] Hedström and Bearman (eds.), *The Oxford Handbook of Analytical Sociology*. See chapter 1, Hedström and Bearman, "What is Analytical Sociology All About? An Introductory Essay", sections 1.2, 1.3 and 1.4, for an outline of structural individualism. See chapters 3 to 10 for categories that analytical sociologists use. For another outline of structural individualism see Udehn, *Methodological Individualism: Background, History and Meaning*, pages 318-319.

Looping

There is a complication that may arise when social scientists use psycho-social understandings that differ from those used by the people being studied, and in particular when they classify people using novel kinds. The people being studied may change their own psycho-social understandings, attitudes and conduct. This may happen when some of the people being studied become aware of how social scientists think of them. They may start to think of themselves as members of the kinds that the social scientists identify, or they may start to refer to those kinds when they think about other people. Examples are ethnic kinds and social class kinds. Once someone sees himself as a member of such a kind, he may start to modify his attitudes and conduct in line with what appears to be expected of a member of that kind. Similarly, someone who sees other people as members of such a kind may expect them to think and act in ways that are associated with the relevant kind, may regard their conduct as explained by their membership, and may only demand further explanations when their conduct conflicts with expectations of members or is unrelated to membership. The new expectations of others may in turn influence people's conduct toward those others. The psycho-social understanding of the social scientists who study the people in question may then need to be modified in order to adapt to such changes. That may lead to further changes in the psycho-social understandings of the people being studied, and so on.

The phenomenon has been noted by Ian Hacking, as a looping effect of human kinds.[37] More general forms of the complex reciprocal relationship between social sciences and the societies studied have also been explored by Anthony Giddens.[38]

When a looping effect occurs, it may have two contrasting effects on our confidence. On the one hand we may fear that the looping effect may make it difficult to regard claims as representing how the people studied would be, independently of the academic studies in question. On the other hand, if the people studied do come to reflect in themselves some of the ways in which social scientists think about them, the claims made by the social scientists may become more accurate than they would otherwise have been.

Incorporation of the physical idiom

The human idiom includes the physical idiom, or at least its non-technical parts, as a component. Discussions of human lives incorporate at least an implicit consideration of such physical facts as location and what people are physically capable of doing. The folk physics, chemistry and biology that are necessary to understand human conduct are incorporated into any psycho-social understanding. The principles of folk science are implicit in the corpora of the natural sciences, or can be extracted from those corpora by processes of simplification and approximation. For example, the equations that describe the motions of different bodies imply the folk physics principle that there are separate bodies in the world which move independently of one

[37] Hacking, "The Looping Effects of Human Kinds", pages 369-374.

[38] Giddens, *The Constitution of Society: Outline of the Theory of Structuration*, pages 348-354.

another and which change course when they strike one another.

2.2.2.2 Content and use

The human idiom includes terms for all of the following.

- Character traits, such as ambition, timidity or an affectionate nature
- Virtues and vices
- Impressions, as when a pressure is described as painful, or a light as dazzling, or a bowl of soup as comforting
- Emotions
- Properties of people, whether transient or long-term, which make circumstances reasons for action; such properties include needs, desires, and concern for others or for the future
- Processes of deliberation and decision, such as weighing the evidence and making up one's mind
- The appraisal of actions, as when an action is described as generous or as shameful
- A sense of responsibility for one's actions
- Features of groups, such as cohesion, leadership and influence

Terms for these things form part of the vocabulary of everyday life. But they also fall within the vocabularies

of disciplines high up the scale, most obviously the humanities but also large parts of the social sciences. There is a transitional region on the scale of disciplines, in which researchers only use a modest proportion of the distinctively human part of the human idiom. Those parts of economics in which mathematical models of the behaviour of consumers and producers are put forward can be like that. The same may be said of abstract conceptualizations of space in human geography.[39]

It is not surprising that researchers use everyday vocabulary. It is ready to hand, it is well-understood, and its terms are embedded in existing psycho-social understandings which are rich in principles that can be used to explain what people do. Its use also allows researchers to produce accounts which, while academic, are ones to which non-specialists can relate in a straightforward way. That is something we expect of the humanities, and to a lesser extent of the social sciences. Even the technical vocabulary of social psychology is easily glossed in everyday terms.[40] It is important that such glossing should be possible. It creates connections with everyday conceptions of the ways in which people think and act, allowing those everyday conceptions to be a source of meaning for the technical terms. It also makes it reasonably easy for disciplines that are still shaped by their long non-technical traditions to make use of results from disciplines that are distinctly technical, because the technical disciplines do not use a vocabulary that is disconnected from non-technical vocabulary. For example,

[39] For economics, any textbook of microeconomics will set out mathematical models of the type to which we refer here. For abstract conceptualizations of space in human geography and related disciplines see Crang and Thrift (eds.), *Thinking Space*; Massey, *For Space*.

[40] See for example the glossary in Hewstone, Stroebe and Jonas (eds.), *An Introduction to Social Psychology*, pages 601-612.

history can make use of social psychology.[41]

The human idiom must make it practical to give accounts of human life, thought and action. It is not merely that the microscopic physical detail of the world must be overlooked. The terms that are used must pick out only a modest number of types of feature of people, so that psycho-social understandings can easily be grasped and put to use. And the concepts to which terms refer must fit together in a way that allows people to give accounts that are explanatory. For example, the concepts of nervousness and caution fit together in a way that allows people to explain someone's caution in relation to new ventures by reference to her nervousness about unforeseen consequences, even though there is no firm rule that nervousness will lead to caution.

Moreover, the concepts need to be ones that make it feasible to state the important features of specific people or societies with reasonable brevity. Concepts of standing character traits are ideal. Concepts of transient psychic or social phenomena may turn out only to be useful when the phenomena occur systematically (for example, every morning or every winter, or every time a given person encounters someone who asserts their superior status), or when they occur rarely. Frequent and unpredictable occurrences would require accounts that were moment-to-moment detailed diaries. Such accounts would be unsatisfactory because they would lead one to lose sight of the overall structure and history of a life or a society.

[41] See for example the papers collected in Glăveanu and Yamamoto (eds.), *Bridging History and Social Psychology.*

Could researchers use a technical idiom?

It might be thought that researchers could renounce our everyday vocabulary and use a purely technical idiom that shared the human idiom's ability to overlook detail, to summarize and to reveal structures. The mere possibility or impossibility of such a move would not have any direct bearing on our main question, which relates to confidence in the claims that actually come to be accepted. Nonetheless, it is interesting to explore the question of possibility. If such a replacement were possible, that would suggest that any loss of confidence that arose from use of the non-technical human idiom would be temporary, and that we could look forward to a brighter future.

We cannot definitively exclude the possibility of a purely technical idiom. There are advocates of eliminative materialism who propose something along those lines as an ambition and who cite indications that it may be achievable, although they would have it achieved within the context of a radical change to our everyday conception of our mental lives.[42]

[42] Examples are Churchland, "Folk Psychology and the Explanation of Human Behavior", section 2; Churchland, "The Neural Representation of the Social World"; Ramsey, *Representation Reconsidered*; Chemero, *Radical Embodied Cognitive Science*. In addition, new work may bridge the gap between the vision of our mental life that neuroscientists have and the vision of it that we have in everyday life. See for example Landreth, "The Emerging Theory of Motivation". In section 9, Landreth considers the relationship between the notion of desire that features in his technical discussion and the commonsense understanding of desire. Another example of work that may come to bridge the gap between neuroscience and our everyday vision is work on the neurological basis of pleasure. See Berridge and Kringelbach, "Towards a Neuroscience of Well-Being: Implications of Insights from Pleasure Research".

There is however no convincing evidence that it would be possible to move to a purely technical idiom that allowed satisfactory explanations of conduct to be given, if we were to require the explanations to be translatable into the terms of anything like our current everyday conception of human conduct. That everyday conception involves both our bodily behaviour and the mental activity that precedes and accompanies the behaviour, with both the bodily behaviour and the mental activity being conceived in everyday terms. An ability to translate would be needed if disciplines conducted in the human idiom were not to be replaced by disciplines that were radically different. We may pick out two obstacles to translation.

One obstacle would be the need to identify features of human beings, presumably characterized in neurological terms, that would suffice to give explanations of human conduct without recharacterizing deliberations and conduct beyond the possibility of translation. This obstacle echoes one of the difficulties that Max Weber identified when he argued against the idea of a study of society and culture on the model of a physical science. In his view, immersion in culture is needed in order to render social phenomena intelligible.[43] If that is so, and if grasp of a technical idiom would not involve immersion, the result of its use would not be an understanding of social phenomena of the type that is given by the social sciences.

A second obstacle would arise because an explanation of conduct in the human idiom that we regarded as satisfactory would be likely to bring a wide variety of information to bear. It might mention the agent's character, experience to date and goals, the external circumstances,

[43] Weber, "Die „Objektivität“ sozialwissenschaftlicher und sozialpolitischer Erkenntnis", pages 172-181, translated as "'Objectivity' in Social Science and Social Policy", pages 73-81.

and the intentions that the agent attributed to other agents. An analysis that linked features of the brain to conduct would not be easy to extend to take in such a wide range of information. Apart from anything else, it is not clear that the attributed intentions of other agents could be captured adequately without recourse to the everyday human idiom. Researchers might pick out neurological features of the brains of the other agents, but the translation between those features and the intentions that the agent being studied attributed to the other agents would need to bring the categories of an everyday psycho-social understanding back into play, in order to have much hope of capturing the effects of that agent's awareness of other agents and their likely thoughts on his own deliberations.[44] For that reason if for no other, the human idiom would remain in play.

Would it be beneficial to use a technical idiom?

It is not at all clear that it would on balance be beneficial to move to a technical replacement for the human idiom.

We may start with an obvious loss. Accounts and claims would no longer be ones to which non-specialists could relate in any straightforward way. Researchers might regard that loss as worth suffering in exchange for the benefits of moving to a technical idiom, the use of which would insulate

[44] The argument that it is necessary to see other people in such everyday terms has been made by Daniel Dennett, who claims that we need to treat others as rational beings with the usual range of mental states if we are to identify their beliefs: Dennett, *The Intentional Stance*, pages 15-17. The conduct of someone who is being studied will reflect his predictions of what other people will do, which will in turn be based largely on his view of what they believe. So if researchers are to understand the effect of his predictions on his conduct, they are very likely to need to characterize what goes on in his head in the terms of the human idiom.

them from the looseness of everyday speech. But that loss is not the only reason to doubt that the move would be beneficial.

Researchers who study human conduct and human societies must take account of the patterns of thought of the people studied. Those patterns of thought reflect the concepts that those people use (or used, if they are historical figures). Researchers can reflect the patterns of thought of the people they study in the accounts that they give by using those same concepts. Such reflection facilitates the drawing of inferences and the formulation of accounts. If researchers' views of the people they study reflect the patterns of thought of those people, researchers can quickly, and probably fairly accurately, work out how the people studied would think about their own situations and what they would be inclined to do. A move to a technical idiom would deprive researchers of that advantage.

We can render unsurprising the scope to reflect the patterns of thought of people who are studied by noting that the human idiom is, in practical life, not only a tool of navigation but also a tool of understanding. It is then but a short step to the academic understanding of human beings.

Our point of departure is the fact that human beings are self-conscious and socially conscious agents. A self-conscious agent can both visualize a situation he would like to bring about and act to bring it about, fully aware that he is himself the agent. Such an agent may very well seek to change himself and his environment in order to prosper. If an agent is also socially conscious, he will be aware of how other people think and of social structures and norms. Such awareness will in turn influence the agent's thoughts and conduct, perhaps in the direction of fitting in and doing what is expected or perhaps in some less

conformist direction. Human beings thus come to take on what John McDowell has aptly called a second nature, a nature that brings with it responsiveness both to ethical demands and to other rational demands that might not be seen as demands from a non-human point of view.[45]

Building on what McDowell says, we can say that the second nature of human beings would have its effects through the medium of the human idiom. People use the human idiom to express their situations and goals, their reasons and passions, their normative positions, and the factors that lead them to choose specific actions. Moreover, the idiom's terms and expressions capture a good deal of the content of the second nature of human beings. Those who speak the idiom fluently, as a language in which they think, thereby have access to the practical understanding that they need in order to navigate the social world. Use of the human idiom as a tool of the academic understanding of human beings is a very near neighbour of that practical use. (Note that it is the idiom that is a tool at this point, rather than a psycho-social understanding itself. We do not need to claim that theory theory, rather than simulationism, gives the correct account of what goes on in practical life. We only need to note that we sometimes talk to ourselves.)

Bernard Williams makes the related point that people and their conduct cannot be fully understood when people are considered purely individualistically. They must be seen in their cultural contexts.[46] Building on what Williams says, much of a cultural context is expressed by those who are embedded within it, and they express their cultural context in the human idiom. They will use that idiom to describe ways of life and habits of thought, and to indicate the bounds of acceptability and the degrees of desirability of

[45] McDowell, *Mind and World*, page 84.

[46] Williams, "Making Sense of Humanity", pages 85-88.

alternative ways of life and habits of thought. They will also use the human idiom to describe the roles of various artefacts and to attribute value to them. Researchers who study a culture, whether in order to understand the individuals within it or for any other reason, find that it can only be described satisfactorily by using the terms of the human idiom. That is the high road to understanding not only the culture in general, but also the preferences of individuals within it. Those preferences need to be understood if the conduct of those individuals is to be explained. A move to a technical replacement for the human idiom would therefore make research very difficult, and perhaps impossible.

2.2.2.3 A narrow range of rational beings

Accounts in the human idiom would only be intelligible to a narrow range of rational beings, comprising human beings and others who were similar to them.

The reason is that many of the concepts that are used when giving accounts in the human idiom derive their content from experience of human life. There might be other routes by which such concepts could come to make sense to rational beings with significantly different ways of life, but we have no reason to think that other routes would be available. There is no reason to think that rational beings with significantly different ways of life could have anything like a human grasp of our concepts of sensations, feelings, motives and personal responsibility. (It is more likely that they could grasp concepts of reasons for action and of decision-making, because many types of rational being would need to work out what to do in various circumstances.) We shall now consider three different reasons why human experience might be vital to

acquisition of the concepts that we use, rather than its merely being the route by which we happen to have acquired those concepts.

One reason is physiological. Human beings sense the world and process information about it in certain ways, on account of the natures of their sensory organs and their brains. Other beings would be built differently. It might be possible to recover the same information from their sensations as from human sensations. One might for example recover the information that a being was standing in front of a tree, the leaves of which were a certain shade of green (defined in terms of wavelengths of light, not in terms of sensations produced by green things). But that would not be the same as recovering the sensations that human beings had. This point stands whether or not there are qualia: affective responses would still be denied to other beings.

A second reason has environmental origins, although its current expression is physiological. It is also a reason that can only be put forward tentatively, given that evolutionary psychology is still developing and that the status of some work in the field is controversial.[47] Despite the controversy, the reason is still worth considering here.

This tentative second reason is as follows. In the past, human beings had to pay attention to certain needs, such as needs to obtain food, find shelter, and avoid the physical dangers that arose in their environment. If they had not paid attention to those needs, they would have become

[47] The pace of development means that reports on the content and the status of evolutionary psychology can quickly look dated. Some classic criticisms can be found in Buller, "Evolutionary Psychology: The Emperor's New Paradigm"; Wallace, *Getting Darwin Wrong: Why Evolutionary Psychology Won't Work*. A response to a wide range of criticisms can be found in Confer et al., "Evolutionary Psychology: Controversies, Questions, Prospects, and Limitations".

extinct. The mechanism to ensure that human beings paid attention to those needs was, roughly, some drives that gave imperatives to act. In the safer times of today, the legacy of those drives is direct recognition that certain circumstances give reasons for certain actions. A drive to keep one's kin out of danger may lead to direct recognition that when someone is in danger, there is reason to save them. A drive to ensure security of food supply may lead to direct recognition that if an alternative job is better paid, there is at least some reason to change jobs. And so on. The contents of concepts such as those of care, courage and security are directly accessible. The corresponding terms get their meanings from life, rather than from dictionaries.

Other rational beings who had evolved in different environments with different pressures might also have developed tendencies that led them to see certain circumstances as reasons for certain actions, but those tendencies would have differed from human ones. Those other rational beings might then fail to see directly that some circumstances were reasons for actions which human beings would see as obviously appropriate. Their environments might for example have given them no reason to develop any concern about the future. Then they would not directly grasp the point of accumulating supplies in order to ensure that one had enough for periods of shortage. The concept of acquisitiveness might then mean little to them. If an account of the history of some human society made use of the concept of acquisitiveness, that account would in turn mean little to them.[48] They could be told that human

[48] An example of such an account, one in which the role of acquisitiveness developed as circumstances changed, is given in Durrenberger and Gillogly, "Greed in a 'Tribal' Economy? Acquisitiveness and Reciprocity in Lisu Society". The point that matters here is that the account given in that paper makes perfect sense to human readers, but might make little or no sense to other rational beings.

beings thought in this way, so that a certain correlation between given circumstances and given behaviour was to be expected, but that would only give a detached understanding of the concept of acquisitiveness, and an etiolated grasp of accounts in which the concept played a role. Human beings would have a far more substantial grasp, because they would see straightaway how certain circumstances made acquisitive behaviour appropriate in the eyes of the acquirers.

A third reason is social. Human beings interact with other human beings. They have developed their psycho-social understandings and the human idiom in ways which allow them to meet that challenge. They use concepts such as those of joy and sadness, ambition and lassitude, openness and deviousness. Such concepts allow them to describe and think about human character and conduct in ways that help them to work and play together, and to navigate round difficult people. Other rational beings living in other circumstances, such as circumstances in which there was minimal interaction or collaboration, would not have derived the same benefit from developing similar concepts, so there is a reasonable prospect that they would not have developed them. Then they would have only a detached understanding of the human concepts, and an etiolated grasp of accounts that were given using them.

We have some evidence from within humanity of the possibility of only being able to have an etiolated grasp of accounts. One illustration is the experience of an anthropologist from the United States trying to explain the story of Hamlet to the Tiv tribe in West Africa. All sorts of things, including Hamlet's right to avenge the murder of his father and the presumed suicide of Ophelia, simply made no sense in a different culture.[49] To the extent that

[49] Bohannan, "Shakespeare in the Bush".

the Tiv tried to grasp European literary culture, they found themselves unable to have more than an etiolated grasp.

There is also the point that several evaluative terms are only properly understood by people who have the interests that support use of the corresponding concepts as guides to action, or who at least see those interests as interests. Otherwise one would not grasp how to apply the concepts in novel situations.[50] It is therefore not surprising that emic as well as etic approaches find favour among anthropologists.[51] But we should not overstate the point. An etic approach need not place an anthropologist in as poor a position as a non-human rational being. One can be more or less etic. If an anthropologist analyses a culture using concepts which, while not reflecting the specific ways of thought of the members of the culture, are nonetheless closely tied to the nature of human life in general, the anthropologist is likely to enjoy a considerably better grasp of the culture than the grasp that would be available to a non-human rational being.

The reasons we have given here why accounts in the human idiom would not be intelligible to a wide range of rational beings are deeply rooted in the conceptual structures of human life. So we should not expect there to be the kind of scope simply to change the presentation of results to suit different sensory and cerebral apparatus that we discussed in relation to accounts in the physical idiom in section 2.2.1.

[50] Williams, *Ethics and the Limits of Philosophy*, pages 141-142.

[51] For these approaches see Barnard, “Emic and Etic”; Berry, Poortinga, Breugelmans, Chasiotis and Sam, *Cross-Cultural Psychology: Research and Applications*, pages 23-24 (of the third edition; pages 291-292 in the second edition). Broadly, emic approaches try to reflect the ways of thought of the people studied while etic approaches prefer the point of view of the anthropologist, whose selection of significant features may well reflect her desire to identify commonalities between the culture studied and other cultures.

2.2.2.4 Bias

Use of the human idiom may lead researchers to formulate accounts of human conduct and human societies which incorporate a bias that would not arise if they used a more neutral idiom. The fact that accounts given in the human idiom would only be intelligible to a narrow range of rational beings encourages us to have this worry. Will claims come to be accepted because they meet standards which are formulated on the assumption that the human idiom should be used, when the bias in the accounts means that the claims should not really come to be accepted?

We have good reason to think that bias is incorporated, because the human idiom is grounded in the practicalities of human life. Human beings have certain perceptual and intellectual apparatus, along with certain physical and social needs, so they think and act in certain ways. In practical life, they must rely on their perceptual and intellectual apparatus to navigate the world and meet their needs. Any psycho-social understanding will reflect those constraints and that task. A psycho-social understanding will not have been developed to be a tool of disinterested enquiry. When researchers use the human idiom to give accounts of human lives and societies, they at least incorporate the bias of psycho-social understandings generally, which is a systematic bias because all such understandings have been developed to meet the needs of human life rather than life in general. At worst, they may incorporate the bias of a specific psycho-social understanding.

The risk that incorrect claims will come to be accepted is however limited, because the claims that researchers make in disciplines that use the human idiom are themselves expressed in that idiom. The claims manifestly characterize human beings in human terms, so any bias that follows

from such a characterization is implicitly acknowledged. Researchers do not pretend to give a view of humanity from some non-human standpoint, let alone from any hypothetical wholly independent standpoint, and it would be inappropriate to apply the standards which would apply if that were pretended. By contrast, claims that are made in the natural sciences are not supposed to be made from a specifically human standpoint. To the limited extent that habits of thought imported from everyday life are used, the risk that work will be biased in ways that may lead to significant error is greater than when the human idiom dominates and that domination is recognized. In evolutionary biology for example, the everyday belief that human beings are quite different from other animals can bias work on human evolution.[52]

We do however still need to ask whether use of a psycho-social understanding that researchers carry over from everyday life to academic disciplines, perhaps with some modifications, is an appropriate route to claims that are expressed in the human idiom. Are everyday psycho-social understandings good at the job they do in everyday life?

There is reason to think they are good at that job. Everyday psycho-social understandings have evolved to work. If they systematically screened out important considerations, they would not work as well as they actually do. Then the failures of human beings when they interacted with other people and with their environment would be a good deal more numerous than they actually are. This suggests that when academic research is guided by psycho-social understandings, important considerations will tend to get noticed.

[52] De Cruz and de Smedt, “The Role of Intuitive Ontologies in Scientific Understanding – The Case of Human Evolution”, pages 358-361.

This is not to say that human beings never overlook important considerations in their practical lives. It is obvious that they sometimes do. Sometimes their failures are very serious, for example when diplomacy fails and war ensues. But we can at least take it that everyday psycho-social understandings do their everyday job reasonably well, and that they therefore provide satisfactory guidance when researchers construct accounts in the human idiom.

We must also recognize the scope to introduce bias by using researchers' own everyday psycho-social understandings, when the psycho-social understandings of people who are studied are or were different. But that risk of bias can be addressed within the range of human psycho-social understandings, by making appropriate modifications to researchers' own understandings.

2.2.2.5 Historical accounts

We may add two considerations that come to the fore when researchers give historical accounts of human conduct. These considerations support the use of everyday psycho-social understandings or modified versions of them.

The first consideration is that everyday understandings contain within themselves the germ of historical thought. Wilhelm Dilthey and Martin Heidegger both explored consequences of the fact that human beings live in time and understand their lives within a temporal framework.[53] This central role of time permeates every psycho-social understanding. People are seen as reacting to things that happened in the past, as planning for events that lie in the

[53] Dilthey, "Die Kategorien des Lebens" ("The Categories of Life"); Heidegger, *Sein und Zeit* (*Being and Time*), sections 61 to 83.

near or the distant future, and as having a sense of the course of their lives laid out in time.

The second consideration is this. In the course of his work on historical understanding, Dilthey claimed that the human sciences rested on the relationship between experience, expression and understanding, and that the development of the human sciences required a deepening of experience.[54] Such claims would suggest, although they would not prove, that the human sciences had to be conducted in terms that resembled the terms of some everyday psycho-social understanding. If they were not so conducted, they would lose touch with human experience. Then it would be pointless to debate whether researchers could do better by standing back and making a choice from a wider range of possible approaches. They might do better if they also radically changed the natures of their disciplines, but that would be beside the point so long as they wished to conduct their existing disciplines.

Even if researchers could conduct the human sciences on the basis of principles that were not derived from everyday psycho-social understandings, we could not imagine their discarding certain key features of everyday experience, such as people's reference to the past, identified as such, in order to steer a course into the future, also identified as such, or people's association of individual streams of conscious thought with individual human bodies.

Unimaginability does not demonstrate impossibility, but it should stop us assuming that there are alternatives to

[54] Dilthey, "Der Aufbau der geschichtlichen Welt in den Geisteswissenschaften", page 131 ("The Formation of the Historical World in the Human Sciences", page 153). It would be difficult to work out exactly which parts of our current range of humanities and social sciences Dilthey would have included in his conception of the human sciences, but it would have been a large proportion.

the retention of such key elements of everyday psycho-social understandings. Moreover, some key aspects of those understandings do reflect features of the world that can be set out in non-human terms. The ideas of reflection on the past and of planning for the future depend on notions of information, time and causation. The idea of individual streams of consciousness being associated with individual bodies, and the idea that different people, whose bodies have been differently placed, will have different pieces of information at their disposal, are both directly connected to the physiology of human bodies, with many direct connections between each person's sensory organs and her brain, and within each brain, but no direct neuronal connections from one body to another.

2.3 Ways to order a scale

We introduced the idea of a scale of disciplines in section 1.5. The idea is not new. For example, Auguste Comte discussed a scale from studies of the simplest and most general phenomena to studies of the most complicated and most particular ones.[55] We shall now look at this idea in more detail. We shall as usual use the word "discipline" to refer both to large disciplines and to sub-disciplines that may be identified within them.

We have a general sense of a scale running up from mathematics to physics and the other natural sciences, then the social sciences, then the humanities. We shall here consider features by reference to which disciplines might be

[55] Comte, *Cours de philosophie positive*, tome 1, leçon 2, and in particular pages 86-87, translated as Comte, *The Essential Comte: Selected from Cours de Philosophie Positive*, chapter 2, and in particular pages 52-53.

placed in order, the extent to which reference to different features would yield the same order, the tidiness of any given scale, and the possibility of constructing a scale by reference to relationships of dependence.

2.3.1 Features that could be used

We might order disciplines by reference to one or more of the features that were mentioned in section 1.4, or by reference to the significance of the human idiom. An order from disciplines low on the scale to those high on the scale might be an order:

- from disciplines in which useful deductive relationships between propositions, or relationships that were almost as strong, were widely available, up to those in which such relationships were rare;

- from disciplines in which pervasive claims played significant roles in constraining what other claims could be made, up to those in which they did not play such roles to any significant extent;

- from disciplines in which claims were typically straightforwardly falsifiable, up to those in which this was not common; or

- from disciplines in which the human idiom was not used at all, up to those in which it was the primary idiom.

2.3.2 Would the use of different features yield the same order?

The features we have suggested would yield similar orders. It is in mathematics and the natural sciences that we are most likely to find deductions, significant constraints imposed by pervasive claims, and straightforwardly falsifiable claims. These are also the disciplines in which the human idiom is not used, except in certain types of psychology. The humanities are likely to lie at the other ends of scales constructed by reference to any of these features, with the social sciences in the middle. All of the orders would be rough, and the details of any one order would be debatable, but the general trends would still be clear.

It is not surprising that similar orders should be found. The features were suggested by some conspicuous contrasts between the natural sciences, the social sciences and the humanities, against the background of a pre-existing sense that there was a scale of disciplines. So the fact that similar orders would arise by reference to any one of a range of different features does not prove anything. The features were chosen with an eye to the order we already had in mind.

It is more interesting to note that there are relationships between the chosen features which make it unsurprising that it is easy to identify different features which give rise to similar orders. If deductive relationships and relationships that are almost as strong are plentiful, that helps some claims to be pervasive by giving them the reach they need to constrain the making of other claims across the discipline. It also facilitates the deduction or near-deduction of empirical consequences of claims, and that helps to make claims straightforwardly falsifiable.

We could not expect orders of disciplines that were constructed by reference to different features to match perfectly, even if we were always able to settle the details of individual orders. There might also be occasions when we could only establish partial orders of disciplines by reference to given features. Fortunately, we shall not need a definitive order of all disciplines and sub-disciplines. We shall not even need to suppose that any fully detailed order exists. We shall only need a reasonable sense of the order of disciplines. And as we noted in section 1.5, it will be safe for us to speak of *the* scale, even given the limitations of any order that might be written out.

2.3.3 The tidiness of any given scale

Although total orders are not necessary, it is interesting to note the reasons why total orders that we would find wholly satisfactory may be unavailable, and the extent to which obstacles to finding satisfactory total orders might be removed by concentrating on small sub-disciplines.

If we arrange traditional disciplines in their entireties on a scale and then look at their detailed contents, we will find overlaps. For example, within the natural sciences physics must come below chemistry, but quantum chemistry should come lower on the scale than geophysics. Within the social sciences, economics must come near the bottom of the range because of its substantial mathematical content, and political science somewhat further up, but the mathematical study of voting systems should come lower on the scale than behavioural economics.

Within the humanities, and to some extent in the social sciences, we may not be able to set out a scale that is indisputably reasonable even when we ignore the detailed

contents of disciplines. Should history be placed higher or lower on the scale than literary studies? Where should sociology be placed relative to the other social sciences? How should geography be placed given its hugely varied forms, both traditional forms such as physical and human geography, and novel forms in which topics include concepts of space?

A partial solution both to the problem of overlaps, and to the problem of the lack of a clear idea of how to order disciplines, would be to replace disciplines as traditionally identified with a much more refined set of sub-disciplines, perhaps the smallest ones that were routinely identified as coherent fields of study.

A focus on small sub-disciplines would reduce the significance of overlaps. For example, those parts of chemistry that one might wish to put lower on a scale than some parts of physics would be separately identified and would be appropriately located below the relevant parts of physics, which would also be separately identified. If we did not then identify the set of sub-disciplines that together constituted physics, nor the set that together constituted chemistry, we would not perceive the overlap that we would perceive if we drew up a scale of large disciplines. Likewise, the more mathematical parts of political science could be identified separately and located below some of the less mathematical parts of economics. Overlaps would not be eliminated entirely, and they would probably increase in number. But their significance would diminish even if there were more of them, because they would arise in relation to comparatively small parts of the totality of academic work. And we would not have lost any information in order to achieve this, because a description of the traditional disciplines and their overlaps could be recovered from the information about sub-disciplines.

A focus on small sub-disciplines could also help where there were no obvious locations on a scale for disciplines as traditionally identified. Sociology and geography could be broken up into their many and varied parts. Many pieces of work in economic history are rich in numerical data and quantitative analysis. This would argue for placing economic history near the bottom of the segment of the scale that covered the humanities. Some other types of history, such as the history of costume, are not generally quantitative, and there would be a case for placing those types of history above computational varieties of literary studies.[56] But even if the focus were narrowed to small sub-disciplines, many sub-disciplines would be found to include some pieces of work that fitted uncomfortably into the places on the scale that were assigned to the sub-disciplines as wholes. For example, there are pieces of work in economic history that are not rich in numerical data or quantitative analysis.

2.3.4 Relationships of dependence

We could order disciplines by relationships of dependence. Discipline D would be placed higher on the scale than discipline E if D made extensive use of results from E, without the dependence being nearly as great in the opposite direction. Thus chemistry makes great use of results in physics, and biology makes great use of results in chemistry, without there being, in either case, dependence in the opposite direction that is remotely as significant. Further up the scale, some historical studies make great

[56]The typical nature of work in the disciplines mentioned may be seen by browsing journals such as *The Economic History Review*, *Costume*, and *Digital Scholarship in the Humanities* (formerly *Literary and Linguistic Computing*).

use of results in economics. As with the features that have been considered so far, the identification of narrow sub-disciplines would be likely to make it easier to give a tidy set of relationships of dependence, although the obstacle to a tidy scale that the identification of sub-disciplines might avoid would be circuits rather than overlaps: parts of substantial discipline X might depend on parts of substantial discipline Y, while other parts of Y depended on other parts of X.

Where disciplines are adjacent, relationships of dependence are clearly visible. Where they are far apart, relationships may be obscured by substantial differences in the conceptual schemes that are used by the disciplines. But they are still present. If the laws of physics that cover the conservation and the transformation of energy had been different, the history of economic development would have been different. If laws of chemistry had been different, making different pigments durable, the history of art would have been different. And it is plausible, but not certain, that if human brains had been different in ways that affected dispositions to act on desires, to cooperate and to compete, economics would have been different.[57]

[57] This last claim is only plausible. It cannot be regarded as established, and this is a useful reminder that we must be cautious when we seek relationships of dependence. There are two reasons why the claim is only plausible. The first reason is that neuroeconomics is an incompletely developed, although rapidly developing, field, and that both its relevance to economics and some of its specific claims are controversial. The second reason is that even if the efficacy of certain ways of analysing economic behaviour can be explained by facts about human brains, it does not follow that if human brains had been different, economics would have been different. Different types of brain might have led to forms of economic behaviour that were apt to be analysed in the same ways, perhaps because the real constraints on economic behaviour were supplied by the nature of the outside world and the consequences of that nature for the efficacy of different ways to obtain resources and render them useful.

A scale constructed by reference to relationships of dependence would not be quite like a scale constructed by reference to one of the other features we have considered – the availability of deductive relationships, the influence of pervasive claims and so on. Those features are intrinsic features of individual disciplines, even if they are most easily seen when we compare one discipline with another. Relationships of dependence are not intrinsic to individual disciplines, but can only exist between two or more disciplines.

Some relationships of dependence will be of interest to us, but we shall not use such relationships to construct a scale. We have plenty of other resources with which to do so.

For a survey of neuroeconomics see Glimcher and Fehr (eds.), *Neuroeconomics: Decision Making and the Brain*. For a line of sceptical thought about the relevance of neuroeconomics to economics as a whole, a line which is not expressly about whether different types of brain would have required different analyses of economic behaviour but which is relevant to that question because it challenges the notion that new insights about the brain could overthrow established economic theories or displace established economic methods, see Gul and Pesendorfer, "The Case for Mindless Economics". For debate over views expressed in that paper and for related issues see the remainder of the volume in which the paper appears: Caplin and Schotter (eds.), *The Foundations of Positive and Normative Economics: A Handbook*.

Chapter 3

Relationships between Propositions

3.1 Introduction

Relationships between propositions play important roles in the processes of making and assenting to claims. Researchers may consider what direct support a claim receives from the evidence and the corpus. Is it for example entailed by a combination of the evidence and contents of the corpus? They may also consider what follows from a claim, in order to see whether the propositions that follow are consistent with the evidence and the corpus. In this chapter, we shall explore types of relationship and the implications for these roles.

3.1.1 Terminology

Propositions are the objects of assertion and denial. The corpus of each discipline comprises propositions. Claims are propositions, and pieces of evidence will be taken to be in the form of propositions. Pieces of evidence that are in non-propositional form will be taken to have been converted into propositions that set out their relevant features.[1]

Propositions p and q may stand in a deductive relationship, with p as premise and q as conclusion. If they do, we shall say that p entails q. Our starting point for the sense of the word "deduction" is the sense that logicians would give it, but we extend the sense in the natural way that is reflected in our everyday assessments of whether relationships are deductive.

Propositions may also stand in a non-deductive relationship, leaving open the possibility of the antecedent's holding while the consequent does not hold. A relationship would

[1] The idea of the conversion of all pieces of evidence into propositional form raises philosophical questions. How for example should an artefact or the sensory impressions received by an observer be described, given that there will be many possible descriptions? How could a description avoid being at the same time an interpretation, which would be likely to affect the significance of the piece of evidence? At the most fundamental level of philosophical concern, what is the nature of the mechanism that relates what human beings sense to what they say? Two essential starting points for the modern discussion of this question, the work of Willard Van Orman Quine and of Donald Davidson, are explored in Dellantonio, "Sinneswahrnehmung und Überzeugungen. Die Frage nach dem empirischen Fundament". But while we note these concerns, we shall not let them detain us. Where enough of a framework is provided by the practice of a discipline, the concepts it uses and the immediate questions that researchers seek to answer, the general ways in which pieces of evidence should be converted into propositional form will be clear enough, even though there may be disputes over specific conversions.

however be of little interest if the correctness of the antecedent were not at least likely to be accompanied by the correctness of the consequent.

In order to make the direction of a relationship clear, we shall sometimes speak of a relationship from p to q. For deductive relationships, this will mean that p entails q rather than the reverse (although q might also happen to entail p). We shall extend this terminology of direction to non-deductive relationships.

3.1.2 Relationships and arguments

Sometimes a claim will be supported by an argument that uses just one or two relationships, as when a piece of evidence shows that some claim is very likely to be correct, or when there is a deductive relationship from the conjunction of some already accepted claims to the claim that is to be supported. More commonly, an argument in support of a claim will be built up in reliance on several relationships between propositions, which will all have their places in the structure of the argument. Support will only be adequate if a large proportion of the relationships are at least reasonably strong.

We shall concentrate on individual relationships between propositions, rather than on the structures of whole arguments. Those structures must of course be examined when considering whether arguments provide adequate support for claims, but the acceptability of any given structure will be determined by ordinary standards of logical argument – including, in most disciplines, standards of informal argument – and by the nature of the discipline's subject matter.

This approach does require us to take a particular view of arguments to support claims. We need to see arguments as built up from propositions that are connected to one another. This is a bottom-up view. It may not reflect how researchers formulate their arguments. They may work in a top-down way. They may first formulate very sketchy arguments that start with evidence and contents of the corpus and end with new claims. Then they may fill in the details, perhaps modifying the new claims as they go along. But once arguments are complete, they can be analysed to expose the component propositions, the relationships between them, and the structures used – although in some disciplines, and particularly those high up the scale, analysis may require considerable re-writing of published arguments and some arguments will not have unique analyses. Given that our interest is in whether claims are well-supported, it is perfectly proper to concentrate on arguments as completed, and not to be concerned that they may have been formulated in ways that our approach would not reflect.

We shall now make some remarks about deductive and non-deductive relationships between propositions. We shall then consider the availability of deductive relationships in different disciplines. As part of our enquiry into availability, we shall discuss the extent to which concepts that are used in different disciplines tend to exhibit indeterminacy of content and vagueness of extension. Finally we shall discuss how the availability of relationships can affect the consideration of whether to assent to claims, and the implications for our confidence.

3.2 Deductive relationships

Deductive relationships can play vital roles in giving support to claims. They may give positive support by creating links from other claims to the claims in question. They may also give support by showing the absence of reasons not to assent to claims, when consequences of the claims in question are deduced and no consequences are found to clash with the evidence or with the corpus.

The notion of a deductive relationship may seem to be straightforward. Progress from one set of propositions to another proposition is deductive if and only if it is made in accordance with an accepted set of rules of deduction. And we may regard the initial set of propositions as a single proposition, the conjunction of all members of the set. But there are some complications to note. We shall start by noting the scope for there to be a choice of rules of deduction. Then we shall consider the key role of the relevant discipline's corpus in allowing deductive relationships to be established. Finally, we shall consider different methods of presentation of deductive relationships.

3.2.1 Choice in rules of deduction

In the view of logicians, a conclusion follows deductively from a set of premises if one can start by writing down the premises in a sequence of lines, and then continue the sequence until the conclusion appears on a line. The rules of deduction must permit each line to be written, given all the lines that precede it. Those rules have to be chosen.

The leading example of choice is this. Intuitionistic logic does not allow use of the law of excluded middle to give

non-constructive proofs, whereas classical logic does allow this. The significance of the choice is that some formulae will qualify as theorems in mathematics that is based on classical logic, but not in mathematics that is based on intuitionistic logic. The choice may have consequences for what can be done in disciplines that are heavily mathematical, in particular in quantum mechanics.[2]

In practice researchers follow the norms of their disciplines, for the good reason that the adoption of those norms has led to successful disciplines. The standard practice in mathematics is to use classical logic. Alternatives are generally pursued for their philosophical interest, rather than because researchers fear that classical logic might lead them astray.[3]

The need to rely on norms reminds us that researchers are not given an indisputable answer to the question of which rules of deduction they should adopt. So there is no absolute fact of the matter, independent of practice, as to precisely which relationships are deductive. But this point should not concern us greatly. The choice between classical and intuitionistic logic is the one really significant choice. And it is not a choice that is likely to have much significance outside mathematics and some types of physics and chemistry.

[2] An example of the use of an intuitionistic approach in quantum mechanics, with an argument that there are good reasons to prefer intuitionism, is provided by Caspers, Heunen, Landsman and Spitters, "Intuitionistic Quantum Logic of an n-level System".

[3] We should however note that if we range more widely than choices of rules of deduction, the philosophical interest of enquiries into alternative conceptions of logical consequence can be substantial. See Shapiro, "Varieties of Pluralism and Relativism for Logic", section 2. For the interest of intuitionism and of placing restrictions on use of the law of excluded middle, see section 6.

Choices of rules of deduction are to be distinguished from the choices of axioms that mathematicians sometimes have to make, although choices of axioms can equally well have great influence on what can be established deductively.

3.2.2 The role of the corpus

The corpus of a discipline plays a vital role in supporting deductive relationships. It is rare for a small set of premises in isolation to lead deductively to a worthwhile conclusion. Most deductive relationships that are worthy of attention will draw on the corpus to supply extra premises. When a relationship from p to q may only be regarded as deductive with support from the corpus, the relationship is really from the conjunction of p and the required members of the corpus to q.

Our confidence in claims that have come to be accepted because they are the conclusions of deductive arguments must therefore be influenced both by the degree of our confidence that the explicit premises are correct, and by the degree of our confidence that claims in the corpus which are implicitly used as premises are correct.

3.2.3 The presentation of deductions

Deductions may be presented in several different ways. We shall note some possibilities here. We shall say more about the presentation of both deductions and non-deductive arguments, and about the significance of presentation for our confidence, in section 3.5.2.2.

The most formal option is presentation in a way that is governed by explicit rules as to what may be written. A completely formal proof is a sequence of lines in which the presence of each line is authorized by rules that govern what may be written, given the contents of earlier lines. Examples of rules are modus ponens and rules of instantiation that may be applied to quantified formulae.

The next most formal option is presentation of a proof in a way that omits some formal manoeuvres because researchers can still follow the argument, and will still accept that it is of the highest quality. For example, the writers of mathematical proofs need not justify every line that they write in the way that would be demanded by formal rules of proof. They need not set out every step in the reasoning in detail.[4]

Finally there are presentations that do not rely wholly on sequences of lines of words and symbols, but that use diagrams or other devices to convey points that researchers can then grasp.

The distinction between the first two forms of presentation is not very significant for our purposes. Proofs are not put in doubt merely because they move faster than a strict code of proof would allow. The important distinction is between these two forms and the third form. And what matters is not the outward forms of presentation, whether as lines of words and symbols or as diagrams. The important distinction is between types of presentation which make it

[4] For an exploration of informal proof see Leitgeb, "On Formal and Informal Provability". Leitgeb's discussion makes it clear that we should not assume that the process of turning an informal proof into a completely formal proof would be easy, or even psychologically possible, even for a highly skilled mathematician. For how a proof that has gaps of certain types can still justify belief in its conclusion see Fallis, "Intentional Gaps in Mathematical Proofs".

easy for researchers to be reassured that no inadequacies would come to light if they tried to formalize the proofs in full, and types of presentation which make it harder to gain that reassurance. It just so happens that with some exceptions, it is easier to gain that reassurance with proofs that are written out in words and symbols than with proofs that are given by using diagrams.[5]

We should also note that when computers are used to construct proofs or check them, that raises special issues. Arguments may be fully formalizable in principle, and may even be fully formalized within the computers, but it may be hard or impossible for human mathematicians to grasp the details in a synoptic view. Our confidence in the conclusions of proofs that computers construct or check depends on our confidence that mathematicians have taken enough care when developing the relevant programs. But mathematicians need not be debarred from having an a priori warrant for believing the results that are produced by theorem-proving computer programs.[6]

[5] Alama and Kahle, "Checking Proofs", explores ways in which reassurance may be gained. The paper emphasizes the importance of arguments being formalizable in principle. For an example of rigorous deduction using diagrams, together with the option of reconstructing diagrammatic proofs as traditional logical proofs, see Urbas, Jamnik, Stapleton and Flower, "Speedith: A Diagrammatic Reasoner for Spider Diagrams".

[6] Alama and Kahle, "Checking Proofs", sets out roles for computer programs that construct and check proofs. Burge, "Computer Proof, Apriori Knowledge, and Other Minds", argues that mathematicians may have an a priori warrant for belief in the results that are produced, although the grounds for belief may be defeasible. This conclusion appears on page 332.

3.3 Non-deductive relationships

Many relationships between propositions are non-deductive. We shall first explore the different degrees of strength that such relationships may have, and then consider the role of the corpus. Then we shall consider the legitimacy of using non-deductive relationships.

3.3.1 Degrees of strength

Some non-deductive relationships are strong enough that correctness of the antecedent would make correctness of the consequent very likely. To return to the Higgs boson example that we mentioned in section 1.3, there is such a relationship from the observations actually made to the existence of the Higgs boson (although in such cases, one should not read p as the probability that the relationship would fail). Other non-deductive relationships are considerably weaker. There may be appreciable scope for the antecedent to be correct while the consequent is incorrect. Then researchers should only say that correctness of the antecedent would make correctness of the consequent likely. For example, there is a relationship of this nature from the antecedent that someone writing about a historical event well before their time used words similar to those used in an account dating from the time of the event, to the consequent that the later writer had read the earlier account.[7]

[7] The example comes from Watson and Cameron, *Cool Britannia: Snowier Times in 1580-1930 than Since*, page 51. It concerns an account published in 1790 of snowfall in January 1634.

3.3.2 The role of the corpus

The corpus plays a role in relation to non-deductive relationships comparable to the role that it plays in relation to deductive ones. It provides enough background to allow the existence of relationships to be claimed and defended.

3.3.3 Using non-deductive relationships

Given the obvious weaknesses of non-deductive relationships as compared to deductive ones when deciding whether to assent to claims, it might be thought regrettable that researchers should rely on them. This is an appropriate point at which to respond to that thought.

In some disciplines, researchers have to make extensive use of non-deductive relationships. There are not enough deductive relationships to allow them to go very far. If researchers limited themselves to deductions alone, and refused to make use of non-deductive relationships, many of the disciplines that are comparatively high up the scale would be very sparse. The justification for using a mixture of deductive and weaker relationships is that disciplines built in that way are successful. The social sciences and the humanities greatly improve researchers' understanding of people and the world. Allowing the use of non-deductive relationships greatly expands the intellectual potential of humanity. There is an increased risk of taking a wrong turn, but that must be accepted in order to make progress at a decent pace.

It is therefore an intellectual virtue of researchers that they can use non-deductive relationships to conduct extended arguments. Doing so is not like conducting deductive

arguments and making mistakes. It is an ability to reason in a variety of forms, not all of which amount to step-by-step progress from premises to conclusions, while recognizing the danger of error and controlling that danger by using good sense and insisting on corroboration for claims. Researchers are well aware of when a claim is adequately supported. They can work in accordance with disciplinary norms, such as norms that concern how evidence must be interrogated. And they can apply general standards of argument, standards that must be observed if arguments are to be regarded as rational.[8]

Researchers can also consider whether the conclusions of an argument fit with their existing understanding. They may draw on detailed information contained within their discipline. Particularly in the social sciences and the humanities, they may also draw on more general resources such as awareness of how human beings and human institutions generally behave.

The existence of these controls, both over standards of argument and over conclusions, means that while our confidence may be diminished by our awareness that much depends on the use of non-deductive relationships, the diminution need not be great. The success of non-deductive reasoning is reassurance enough. We may gain further reassurance from the fact that some non-deductive

[8] Such standards are discussed in Johnson, *Manifest Rationality: A Pragmatic Theory of Argument*, particularly chapter 6 on the nature of argument, chapter 7 on standards of argument and chapter 8 on the process of criticism. Johnson is concerned with arguments that exemplify not only processes of inference, which he calls the "illative core", but also responsiveness to criticism, which he calls the "dialectical tier". An academic paper might contain only an illative core, a network of inferences. Nonetheless, researchers could apply considerations such as those set out by Johnson in order to consider whether the paper's argument was satisfactory.

reasoning can be brought within a rigorous formal structure by using adaptive logic.[9]

3.4 Availability

In this section, we shall consider the availability of deductive relationships between propositions in different disciplines. Deductive relationships are on the whole more readily available in disciplines that are relatively low down the scale.

We shall consider two main reasons why the availability of deductive relationships varies as between disciplines. The first reason is that the complications of the objects of study make it harder in higher disciplines to make use of exceptionless laws of nature as supports for deductive relationships which are at a level appropriate to the discipline. The second reason is that the ways in which content is given to concepts differ as between lower and higher disciplines. The ways to give content that are used in higher disciplines do not tend to facilitate the identification of deductive relationships.

[9] The use of adaptive logic is set out in Straßer, *Adaptive Logics for Defeasible Reasoning: Applications in Argumentation, Normative Reasoning and Default Reasoning*. The main idea is explained in chapters 1 and 2. There is no guarantee that all respectable reasoning that was non-deductive could be brought within the scope of this project. Straßer's approach is designed for reasoning in which usual conclusions sometimes fail to follow from premises because of abnormalities that are at least in principle specifiable − for example the possible abnormality of Tweety's being a penguin which prevents "Tweety flies" from following from "Birds fly" and "Tweety is a bird" (page 3). Reasoning in which failures of conclusions to follow deductively did not reflect specifiable abnormalities would be likely to require a different approach.

We shall then move on to a third and less significant reason, vagueness in the extensions of concepts. Vagueness of extension can make it hard both to establish deductive relationships, and to make full use of such deductive relationships as there are. It is a greater problem in higher disciplines than in lower disciplines.

We shall speak of concepts rather than terms. We shall regard concepts as having contents, corresponding to the meanings of the corresponding terms. The content of a concept is what one might reasonably conclude about an object if one were told that the concept applied to it. To be a bit more specific, the content would often need to be a function from various bodies of information about an object to various sets of conclusions about the object, since what one would conclude might well depend on other information one had about the object. (A more precise specification of the notion of content, replacing "what one might reasonably conclude" with something more specific, might be needed for some purposes, but not for ours. Indeed, it is not clear that a more precise specification could be given in the current context without making the notion of content unsatisfactory in some other respect.)

The preference for speaking of concepts rather than terms reflects our concern with the contents of claims that are made, rather than with the language in which claims happen to be expressed. There would be a case to be made for speaking of terms, based on the fact that our concern is with relationships between propositions, since propositions must be expressed in some language or other, and relationships must be established by considering what has been stated in the chosen language. But given our purposes, nothing hinges on the choice. And those who would prefer a discussion that referred to terms would be able to re-cast what follows to suit them, at least if they

assumed a one-to-one correspondence between concepts and terms in some specified language.

3.4.1 Complications

The world as viewed by physics comprises objects that are individually very simple. There are few things to say about a particle, or a field, or a macroscopic object considered as a rigid body that has a centre of mass and is subject to forces. The mathematical characterizations of such objects and their behaviour may be sophisticated, and decidedly non-intuitive to most people, but nonetheless all the relevant properties of objects can be set down together. As we move up to chemistry, we find that the complications of molecules make such comprehensiveness harder to achieve. If we turn to geology we find that while the features of interest, whether strata or stresses, may be simply defined, those features only appear in very large assemblies of molecules. Geological concepts must be used, and descriptions that are given using those concepts do not capture the full physical detail of the world. Moving on to biology, we find that even individual cells are enormously complicated. Specifically biological concepts must be used, and descriptions that are given using those concepts overlook a great deal of physical detail. Finally, the world as viewed by the social sciences and the humanities comprises immensely complicated objects, in particular human beings. The objects of study can only be made tractable by speaking in the human idiom and using the corresponding concepts, the application or non-application of which to objects discloses very little of the physical detail of the world. (Some physical detail may be disclosed, as when a description of a person sets out a compound of physical characteristics, physical situation

and mental state, but the disclosure of physical detail is not the main business of characterizations that are given in the human idiom.)

The notion of complication must be interpreted broadly. It is not limited to the presence of many particles in the world in many patterns. It also covers the presence of intricate feedback loops, for example the loops in ecological systems, or the loops in human beings when they reflect on how others see them and modify their own conduct accordingly.

We shall now explore why complication should reduce the availability of deductive relationships. Exceptionless laws of nature are mostly only available at low levels, and they cannot be used to support deductive relationships at a level appropriate to a high-level discipline (for example, relationships between propositions that are set out in the terms of biology or economics rather than in the terms of physics – in this section, "high-level discipline" will mean any discipline higher than physics or chemistry). It is support that matters. If there were in fact some deductive relationship, but researchers could not support its existence, it would not be available for them to use in their work.

An alternative to relying on low-level laws to support high-level deductive relationships would be to find exceptionless high-level laws. In section 3.4.1.2, we shall discuss why that alternative is for the most part unavailable.

3.4.1.1 Manageable accounts and laws

The accounts that disciplines give need to be manageable. When the objects of study are as complicated as geological formations or cells of organisms, the detail that would be given by a particle-by-particle account, or even a molecule-

by-molecule account, must be overlooked. If the objects of study are human beings or societies, much more detail must be overlooked. It is therefore necessary to move on to new concepts that are appropriate to geology, biology, psychology, sociology or whatever the discipline may be.

This move means that researchers lose the ability to use exceptionless low-level laws to support deductive relationships at levels that would be appropriate to their disciplines (for example, deductive relationships between propositions that attributed biological or psychological states to entities). Low-level laws could only be used to support deductive relationships between propositions in high-level disciplines if the antecedent and consequent propositions could be translated into propositions that used the concepts of low-level disciplines, propositions of the types that were linked by the low-level laws. Schemes of translation of the required type, comprising substantial sets of rules of translation, would at the very least be impractical.

We need to clarify what is being argued here. It is that there is, at least in practice and perhaps in principle, a conceptual gulf between high-level disciplines and low-level disciplines. It is the conceptual gulf that matters here, because our concern is with the unusability of exceptionless laws of nature to support deductive relationships in high-level disciplines. If the conceptual gulf could be bridged, there would be the further obstacle that even when researchers saw clearly that some concept of a high-level discipline applied to some object of study, that would not give them the access to physical detail they would need in order to make low-level laws usable. But we do not get that far. And the consequences of loss of detail are more interestingly explored by asking why it is very hard to formulate exceptionless laws within high-level disciplines themselves. We shall consider that question in section 3.4.1.2.

It is manifest that schemes of translation of the required type, allowing moves from the concepts of high-level disciplines to the concepts of low-level disciplines, would not be available. Occasional rules of translation might be available, but reasonably comprehensive schemes of translation would be needed if researchers in high-level disciplines were to make use of exceptionless laws in low-level disciplines to support deductive relationships. When high-level disciplines describe the circumstances and conduct of people in human terms, we may expect there to be obstacles in principle, and not merely in practice, to comprehensive schemes. One such obstacle is this. Descriptions of people's circumstances and conduct in human terms seem to be prime in Timothy Williamson's sense: not decomposable into the conditions of individuals' minds and of their environments at particular moments.[10] But decomposition would appear to be an essential first step in the formulation of a scheme of translation.

We have put the point in terms of concepts and their translation, but there is an obvious connection with the wider debate about scope for reduction. It is clear that a full reduction of disciplines that investigated complicated objects to physics would be impractical, whether or not such a reduction might theoretically be possible.[11] Even when reduction appears to receive direct support from experimental results, we need to be cautious about drawing general conclusions. It is important to consider the designs and protocols of the experiments.[12]

[10] Williamson, *Knowledge and its Limits*, chapter 3.

[11] Recent discussions of views on the nature and the possibility of reduction can be found in Dizadji-Bahmani, Frigg and Hartmann, "Who's Afraid of Nagelian Reduction?"; Endicott, "Reinforcing the Three 'R's: Reduction, Reception, and Replacement".

[12] Sullivan, "The Multiplicity of Experimental Protocols: A Challenge to Reductionist and Non-Reductionist Models of the Unity of Neuroscience".

One trend in recent decades has been to seek not perfect reductions, but reductions of high-level theories to low-level theories which can in turn be used to build back up to approximations to the high-level theories, rather than to exact reproductions of those high-level theories. This allowance for approximation certainly improves the prospects for reduction when a high-level theory is given within a discipline that is reasonably low on the scale, for example in chemistry. But higher up the scale, the allowance is not likely to be much help. Moreover, when we move up to psychology of all but the most neuronal sort, or to the social sciences, researchers very often do not even have theories that would be well-defined enough to allow any project of reduction of theories to begin. There may still be scope to identify underlying physical mechanisms which in fact allow functions that are characterized in high-level terms to be performed.[13] But even though that may be possible, such case-by-case (token) reductions would hardly be enough to support a general programme of reduction that could even come close to being a reduction of theories. And any weakening of the notion of reduction to the mere identification of physical mechanisms that performed defined functions would be a significant weakening.[14]

Finally, while the most obvious barriers to reduction arise when one considers disciplines that are reasonably high up the scale, barriers can arise as low down the scale as chemistry. An example is given by attempts to use quantum mechanics to explain some properties of molecules.[15]

[13] Endicott, “Reinforcing the Three ‘R’s: Reduction, Reception, and Replacement”, section 9.

[14] For a discussion of functions and reduction in the context of biology see Weber, “Life in a Physical World: The Place of the Life Sciences”.

[15] Hendry, “Philosophy of Chemistry”, sections 3 and 4.

None of what we have said should make us think that laws of nature become irrelevant as we move up the scale of disciplines. All laws of nature still apply even in relation to large and complicated objects, accounts of which must be given using the concepts of high-level disciplines. Moreover, researchers can still make use of some laws of nature in their accounts without going via analyses in terms of simpler objects. They can for example analyse the performance of a high-jumper by using Newtonian mechanics and by noting the scope for an athlete to curve her body so that her centre of gravity passes under a bar that she clears.[16] And when the objects of study are human beings, certain ways to think about aspects of the world may be particularly helpful in suggesting how connections might be made between on the one hand conceptions of people and societies in human terms, and on the other hand biological or neurological conceptions. Such connections might make it possible to see, at least in principle, how laws of nature were all that ultimately mattered, even though it would remain impossible to say in particular cases precisely how they were all that mattered (as distinct from having some general conception of how they were all that mattered). Dan Sperber's idea of an epidemiology of representations as a way of connecting cultural phenomena with what goes on in the brains of individuals would be an example of such thought about an aspect of the world.[17]

3.4.1.2 Exceptionless high-level laws

We shall now consider why researchers in high-level disciplines cannot as a rule identify and use exceptionless laws

[16] Barrow, *Mathletics: A Scientist Explains 100 Amazing Things About Sport*, chapter 10.

[17] Sperber, *Explaining Culture: A Naturalistic Approach.*

at high levels to support deductive relationships between propositions, as an alternative to using exceptionless laws taken from low-level disciplines.

The difficulty lies in the multiple realizability that is consequent on overlooking microphysical detail. Researchers in a high-level discipline will describe a situation using propositions that could be made correct by any one of a variety of situations as characterized in microphysical terms. If they made deductions from those propositions by combining them with laws that had been formulated within their own discipline, the deductions might fail to hold because the actual situation might be an exceptional one. There would be cases in which the microphysical characterization of a situation would bring it within the scope of the antecedent of a high-level law, while the working out of the laws of physics would lead to a situation with a microphysical characterization that would take it outside the scope of the consequent of the law.[18] There would be a risk of this happening even as low down the scale as chemistry and some types of physics, which we are not treating as high-level disciplines, but the risk would be much greater from biology upward.

Noting the exceptions might not require a microphysical description of the situation. It might suffice to add provisos based on a range of special features of situations, where those features could be picked out using concepts that were appropriate to the relevant high-level discipline. But this remedy would be likely to be an impractical one, because it would require the addition of an unmanageable amount of detail to the characterizations of actual situations. An example of the difficulty of taking account of exceptional

[18] Papineau, "Physicalism and the Human Sciences", queries the extent to which multiple realizability is the problem that it is made out to be. But he does not establish that it is not a problem.

situations is supplied by the transport of molecules within cells. Their diffusion does not obey the obvious models, but factors drawn from a wide range may need to be mentioned in order to explain what happens in particular cases.[19]

The difficulty of taking account of exceptional situations would be even greater than consideration of the immediate objects of study might suggest. Differences between situations might lie either within those objects or in their environments. When we consider redescription in microphysical terms, it is important to take environmental differences into account for two reasons. The obvious reason is that environments influence the behaviour of objects within them. The less obvious reason is that environmental differences need to be noted in order to maintain straightforward one-many relationships of multiple realizability, and not to be faced with many-many relationships in which identical low-level characterizations of objects in isolation might correspond to different high-level characterizations because of environmental differences that were taken into account when giving high-level characterizations.[20] It would be just as important to take environments into account when exceptions were to be handled not by using microphysical descriptions, but by using concepts appropriate to the relevant high-level discipline to pick out special features of situations.

Even if researchers did not actually find exceptions to high-level laws, they would be aware that exceptions could arise. The researchers therefore could not regard the laws as giving them deductive routes from descriptions of

[19] Höfling and Franosch, "Anomalous Transport in the Crowded World of Biological Cells".

[20] The way in which many-many relationships could arise is explained in Khalidi, *Natural Categories and Human Kinds: Classification in the Natural and Social Sciences*, pages 119-120.

antecedent situations that were stated using the concepts of their own discipline to descriptions of consequent situations that were stated in the same terms.

That much indicates why researchers cannot usually write exceptionless high-level laws from scratch. But why can the exceptionless nature of laws at the microphysical level not be transmitted up to laws at higher levels?

When researchers give accounts of large and complicated objects, the laws they can state, laws that relate to those objects as described in the relevant disciplines rather than as described in microphysical terms, often reflect the confluence of various exceptionless laws, or of laws subject to exceptions where those exception-prone laws themselves reflect confluences of various exceptionless laws. A law that items of pottery break when dropped onto concrete from a height of one metre reflects the confluence of a law about acceleration due to gravity, laws about the dissipation of kinetic energy through bodies on impact, and laws that govern changes to chemical bonds when clay is fired. Laws concerned with changes to chemical bonds are subject to exceptions when the proportions of ingredients vary, or when the firing temperature is higher or lower than normal, but they will be underpinned by general laws of chemistry that are exceptionless or very nearly so. To take an example from economics, a law that the utility to a given consumer of each extra unit of a given product would diminish, either from the first unit onward or beyond some level of consumption that was itself accessible in practice, would be underpinned by laws of human physiology and psychology that were themselves prone to exceptions. Those laws would in turn reflect confluences of various exception-prone laws of biochemistry and neuroscience. Researchers might ultimately trace a way back to exceptionless laws, but only through a very complex and tangled web.

A complete tracing back to exceptionless laws would nearly always be impractical. There is also scope to debate whether exception-prone laws supervene solely on exceptionless or near-exceptionless laws. But we can still say that such a tangled web of support is a significant source of the exception-prone laws that researchers can state when they give accounts of large and complicated objects. It is not plausible that exception-prone laws would be reliable enough to be of any interest in the absence of a body of laws that were exceptionless or very nearly so. Having said that, it would not be necessary to be aware of the details of those exceptionless or near-exceptionless laws.

This dependence on a complex web would make it very difficult for researchers in high-level disciplines to formulate laws with exclusions for special cases to the extent that the exclusions would render the laws even close to exceptionless. The complexity of the web would make it very difficult to work out in advance what exceptions would arise. Exceptions arise when laws interact in odd ways, or when some inhibitor comes into play in a way that may be very hard to anticipate.[21] Even if researchers could give laws with exclusions that would render the laws close to exceptionless, they might well find that the laws were impractically

[21] We may compare this with the view that some failures of laws in the higher sciences should be attributed to failures of the combinatorial laws that determine the resultant of a number of individual causal influences: Rupert, "*Ceteris Paribus* Laws, Component Forces, and the Nature of Special-Science Properties". We shall consider a closely related issue in section 5.2.3.6.

Another important point of comparison is the work of Luke Fenton-Glynn on minutis rectis laws, laws that only hold so long as the microscopic dispositions of particles are not exceptional: Fenton-Glynn, "*Ceteris Paribus* Laws and *Minutis Rectis* Laws", section 3. Fenton-Glynn does however apply this notion to laws that are right down in physics, and in particular to laws of thermodynamics, although he does not limit application of the notion to laws within physics.

cumbersome. And even if that obstacle could be overcome, there would be the further difficulty of detecting when an instant case was one to which an exclusion applied, especially if detection required attention to large quantities of fine detail.

Consequences

Laws with exceptions do not have as much power as exceptionless laws to support deductive relationships between propositions that describe specific situations.

Laws that are subject to exceptions may still be able to support some deductive relationships. It may for example be clear that whenever one law applies another one will apply, supporting a deduction from the applicability of the first law to the applicability of the second law. That is, it may be clear that all exceptions to the second law are also exceptions to the first law, even though it may not be possible to identify exceptions to either law in advance, whether identification means defining the exceptions in general terms or recognizing instances that would be exceptions.

Moreover, laws that are subject to exceptions may be able to support near-deductive relationships between propositions that describe specific situations. Researchers are not frustrated by exceptions at every turn, and disciplines do not collapse into the chaos that would follow if researchers were unable to make any reasonably reliable general claims. If we consider how information is lost as we move up the scale of disciplines, we can see why reasonably reliable general claims can be made and why we should not expect a collapse.

In a discipline that is not near the bottom of the scale, initial situations are characterized in ways that overlook detailed physical information. The conditions of application of laws within the discipline are themselves set out in the same kind of way, with detailed information overlooked. The consequences to which laws point will also be set out in this way. Then a law will only lead researchers to expect something that does not transpire if exceptional circumstances, not noted in the description of the initial situation, create an actual consequence that is described differently from the predicted one when both the actual and the predicted consequences are described in the discipline's usual high-level terms, overlooking detailed information. When actual and predicted consequences are described in the same way in high-level terms, that will not count as an exception to the law.[22]

Very low down the scale, detail is not overlooked or is only overlooked to a very modest extent. Initial situations and expected consequences are simple enough to be described in ways that capture all or most of the information about them. The conditions for the application of laws can also be given comprehensively. The comprehensive specification of consequences might be expected to increase the risk of exceptions to laws, but the comprehensive specification of initial situations and conditions for the application of laws ensures that this is not so.

The risk of exceptions would not quite reach zero, even in fundamental physics, but that would be because current theories might be in some way incorrect. That risk arises at

[22] Compare Woodward, "Causation with a Human Face", section 4.5. As Woodward points out, coarse-grained specifications, both of causes and of their effects, make it possible to establish relationships between causes and effects that are more stable than the relationships that would obtain between incomplete fine-grained specifications (pages 88-89).

any level. It is distinct from the risk of exceptions to laws that arises from the loss of information. It is however related to that risk, because one way for a theory to be incorrect is for it to fail to recognize the salience of information that is disregarded under the terms of the theory.

The remarks made here are independent of whether laws express the consequences of situations for individual objects or the consequences for population statistics. Consequences may be expressed in statistical terms in fundamental physics for deep theoretical reasons. They may also be expressed in statistical terms in other disciplines, albeit for rather different reasons. For example, the Hardy-Weinberg law on the constancy of allele and genotype frequencies down the generations in the absence of disturbances is a law about proportions in successive generations, not about the genetic make-up of any given individual. Statistical consequences may be fitted into the conception of the effects of multiple realizability that has been set out here, just like consequences that are not expressed statistically. But one special feature of statistical consequences is that researchers cannot be quite sure whether they have arisen, whether in normal or in exceptional circumstances. The reason is that observed data from a sequence of measurements might have arisen because the elements in the sample happened to be unrepresentative of the population, or because a population at the time of measurement happened to be unrepresentative of the population at most times. Researchers may however be able to exclude beyond reasonable doubt the possibility of a sample, or a population at a particular time, being unrepresentative. They may therefore be able to say beyond reasonable doubt whether an expected statistical consequence has arisen.

3.4.2 Ways to give content

We shall now turn to ways in which the contents of concepts may be given. As we said in section 3.4, the content of a concept is what one might reasonably conclude about an object if one were told that the concept applied to it, making adjustments to what one might conclude for other information about the object. The content of a concept may be more or less determinate.

In disciplines that are low down the scale, the content may well be fully determinate. (We shall use the expression "fully determinate content" for content that either exhibits no indeterminacy at all, or comes very close to that ideal.) It will be clear what may be concluded about an object on the basis of the information that a concept applies to it, together with other information. For example, there may be some truth function of conditions like a disjunction of conjunctions of conditions, such that the concept applies if and only if all the conjuncts of a disjunct hold.[23] And additional information might determine which disjunct held, allowing more to be concluded than merely that some disjunct or other held. Moreover, it might be clear that within the scope of the relevant discipline, nothing more was to be concluded about an object than what followed from the precisely defined contents of the concepts that applied. Clarity about what followed from the application of concepts, and clarity that nothing more was to be concluded, would combine to give fully determinate contents to the concepts involved.

Higher up the scale, indeterminacy of content is more likely. Concepts generally do not have their contents specified precisely, and even when more information is supplied it

[23] The model here is the inus conditions in Mackie, *The Cement of the Universe: A Study of Causation*, chapter 3.

may be unclear how much may reasonably be concluded about an object on the basis that a concept applies to it. What, for example, might reasonably be concluded about a political party on the strength of the information that it was right-wing, even when a fair amount of information about the politics of the party's country was also supplied? It is no response to say that researchers should limit themselves to the minimal core content that could safely be inferred. That would leave many concepts with minuscule contents. It would then be very hard to make progress in the relevant disciplines. What may reasonably be concluded needs to be more than what could be concluded with complete confidence. Turning to the upper bound on what may be concluded, there is unlikely to be any well-defined point at which it becomes clear that nothing more should be added. So indeterminacy will arise at both ends. There will be a lack of clarity about what follows from the application of concepts, and a lack of clarity as to the limit on what may be concluded.

If the contents of concepts used within a discipline are generally fully determinate, then it will usually be clear what to conclude about an object when told that a concept applies to it. That will in turn facilitate the establishment of deductive relationships between propositions. At the most basic level, it may be established that if one concept F applies to an object, then it will have properties that suffice for some other concept G to apply to it. Then for any value of x, there will be a deductive relationship from "x is F" to "x is G". If on the other hand it is not clear what to conclude about an object when told that a concept applies to it, it will be harder to establish deductive relationships in that way.

This is however a point about isolable relationships between specified propositions, rather than about the strength of

extended arguments. Indeterminacy in the contents of concepts need not be fatal to the prospects for extended arguments that create very strong links from premises to conclusions, because multiple connections in a complex argument can mean that whenever indeterminacy of content might create a weakness, strength elsewhere in the argument would make up for the weakness.

Since we wish to identify obstacles to the establishment of deductive relationships, we are equally concerned both with indeterminacy that results from the lack of full and precise specification of content, and with indeterminacy that results from disagreement among researchers as to what the content should be. Such disagreement is just as much an obstacle as incomplete or imprecise specification, even if each researcher is wholly convinced of the merits of her preferred specification of the content, because it may prevent agreement as to which deductive relationships may be used.

The determinacy of a concept's content can depend heavily on the way in which the content is given. We shall now consider different ways in which content may be given, the extent of their use in different disciplines, and the effects on determinacy of content. We shall work our way from methods that may well allow significant indeterminacy to methods that are likely to yield full determinacy. In so doing, we shall work our way from methods that are conspicuous in higher disciplines to methods that are conspicuous in lower disciplines.

3.4.2.1 Giving examples

Researchers may give examples from a concept's extension, then add an explanation of why the examples have

been grouped together and why other objects have been excluded. In identifying the qualities that explain the grouping and the exclusions, researchers give content to the concept. It is important to indicate some qualities, unless they would be obvious to any reader. If examples were merely listed, with no mention of qualities, that would barely give any content at all.

This method is not to be despised. Sometimes, it is the only way for researchers to indicate what they mean. A historian, for example, could not give a generally acceptable statement of the content of the concept of revolution in any other way. But this is clearly no way to make the content of the concept determinate.[24]

There is a way to use examples to give content with precision. This is to construct artificial examples that emphasize certain features and are shorn of the messy details found in real examples. This approach merges into Max Weber's method of the use of ideal types.[25] But claims reached as conclusions of arguments that use concepts which have been given content by using artificial examples are only respectable if there is an additional stage of checking that the artificial examples bear appropriate relationships to the world. If the examples are constructed well, they may bring out the essence of the world. If they are constructed badly, their use may lead researchers to a distorted picture of the world.

[24] For the fortunes of the concept of revolution see Koselleck, "Historical Criteria of the Modern Concept of Revolution". Kroeber, "Theory and History of Revolution", considers how to define the term "revolution" (on pages 24-25), and the scope for theorizing about revolutions. There are further thoughts on definition in Pincus, "Rethinking Revolutions: a Neo-Tocquevillian Perspective", section 1.

[25] For a survey of Weber's various comments on ideal types and the implications of his comments see Bruun, *Science, Values and Politics in Max Weber's Methodology*, chapter 4.

Finally, we have spoken of examples, but even if the entire extension of a concept were catalogued, that might not yield a fully determinate content. It might not be clear what properties would jointly yield a condition of membership that would be satisfied by all and only the members of the extension.

3.4.2.2 The history of use

Concepts may be given content by the history of their use, covering both how they were first used and how their uses have evolved. The contents of some concepts, particularly those that relate to human emotional, ethical and political life, can only be grasped if one understands the history of the concepts. One cannot for example properly grasp the content of the concept of sovereignty without a grasp of that concept's history.[26] There has indeed been for several decades a substantial programme of work to set out the histories of concepts, and to draw from those histories not only what concerns us here, the contents of concepts, but also some wider historical lessons. This work has been carried on under the general heading of conceptual history, although the corresponding German term, Begriffsgeschichte, is also used in English because German historians have been leading practitioners.[27]

The history of use of a concept is not likely to be the only source of its content. There will normally be a dictionary definition that will give a starting point. But often in

[26] See Jackson, *Sovereignty: Evolution of an Idea*, for the use of history in understanding this concept.

[27] For guides to this area of work see Richter, *The History of Political and Social Concepts: A Critical Introduction*; Hampsher-Monk, Tilmans and van Vree (eds.), *History of Concepts: Comparative Perspectives*.

the humanities, and sometimes in the social sciences, a dictionary definition is inadequate to capture the full content of a concept. The history of use may then make a substantial contribution.

It may do so by showing how different elements have been incorporated in or ejected from the content of the concept over the years. The evidence will be found in the arguments that use the concept and that have been accepted or rejected in the past. The history of use may also make a contribution to the content of a concept by giving examples of objects that have been taken to fall within its extension. When researchers use examples as a way to give content, they need not confine themselves to examples that they would themselves regard as members of the extension. They can also draw on the past.

Giving content by reference to the history of a concept's use is only sensible if the aim is to establish the content of the concept at some reasonably specific time – either the present, in order to use the concept in current argument, or some point in the past at which the concept had a role to play, in order to understand what was going on at that time. But even given this restriction to determining the content of a concept at some specified time, the history of use is not likely to be enough to allow the concept's content to be made fully determinate. Different people will have used a concept in different ways and for different purposes, even at the same time in the past. And if one takes account of a substantial period of history leading up to a time for which content is to be given, there may well have been a wide variety of uses of the concept as the intellectual and social context changed. There may even have been significant discontinuities, for example when rhetorical attempts to change the extensions of evaluative concepts have both succeeded and changed the ways in which the concepts have

been used.[28] When an event that created a discontinuity occurred, it may not be appropriate simply to add the history of use before the event to the history of use after it when seeking to establish the content of the concept at some time after the event. But there will be no single correct way to make allowance for the discontinuity. That will make it harder than it would otherwise have been to make the content of the concept determinate.

3.4.2.3 Use in a pattern of thought

Concepts may be given content by their use in patterns of thought within the current practice of a discipline. The diversity of uses that are made of a concept, and the different ways of thinking about topics that are likely to be found in a living discipline that is any distance up the scale, mean that this method is unlikely to yield fully determinate content for any given concept except in disciplines that are very low down the scale. We shall take one example from biology and one from the social sciences.

The concept of a gene is not as well-defined as popular discourse might suggest. One lay definition would be that a gene was a stretch of DNA that was related to some heritable characteristic. This would not be nearly enough to specify the content in a way that would meet the needs of biologists. What counts as a heritable characteristic? Does everything in a stretch of DNA count as a gene, or only the parts that perform specific types of function? Must a gene have independent influence, or can it have its influence jointly with other genes? And so on. The concept of a gene is given real substance by its use in a pattern

[28] Skinner, *Visions of Politics: Volume 1, Regarding Method*, pages 182-186.

of thought, the thought of researchers about what goes on when information encoded in DNA is passed on from parent to child, when that information is put to work in the life of a single organism, or when a characteristic spreads through a population or dies out. Reflection on that pattern of thought may lead to doubts among researchers as to whether the concept of a gene is really a useful one at all. But to the extent that it is retained, its content is given at least partly by the uses researchers make of it. Formal definitions are recognized not to suffice.[29]

To take an example from the social sciences, the concept of path dependence is closely tied to its roles in thought about economic, social and political change. But there is plenty of disagreement, both about the conditions for application of the concept and about what should be concluded when it applies.[30] The extent of disagreement shows that the content of the concept has not been made fully determinate.

It should not be supposed that there are no advantages to giving the contents of concepts by giving examples, considering the history of their use or identifying their

[29] For a history of the concept of a gene and a survey of debates see Rheinberger and Müller-Wille, "Gene Concepts". For a survey of philosophical issues see Griffiths and Stotz, *Genetics and Philosophy: An Introduction*. For deficiencies of formal definitions see Axelson-Fisk, *Comparative Gene Finding: Models, Algorithms and Implementation*, section 1.5. For complexities of the subject matter that impede the giving of formal definitions see Pearson, "What is a Gene?". For three concepts of a gene, two of which (the instrumental and the nominal) would imply that the content of the relevant concept would have strong connections with the patterns of thought of researchers, see Griffiths and Stotz, "Genes in the Postgenomic Era". Different concepts of genes are taken up again in Griffiths and Stotz, *Genetics and Philosophy: An Introduction*, with the instrumental concept discussed under the name of the Mendelian gene.

[30] Mahoney and Schensul, "Historical Context and Path Dependence".

uses in patterns of thought. The scope to emphasize different examples, different elements in the histories of concepts or different strands in patterns of thought allows researchers to experiment with the contents of the concepts they use. Even within the natural sciences there are occasions when conscious decisions not to give the contents of concepts by imposing precise definitions, but to allow classifications and relationships between classes to evolve by reference to samples and the general pattern of thought in the discipline, can facilitate progress. The classification of protein structures provides an example. As more information about structures was discovered, it was found that a rigid treelike map of structures was no longer helpful. A more flexible network of protein structures, in which evolutionary relationships were represented as well as structural ones, and in which examples played a key role in giving the contents of the concepts that corresponded to different classes, was introduced in the expectation that this would aid research.[31]

3.4.2.4 Formal definition

In disciplines that are low down the scale, researchers introduce some concepts simply by defining the corresponding terms. The definitions use terms for existing concepts, where those concepts already have fully determinate contents. The definitions are then taken to give the entire contents, or at least the entire central contents, of concepts.

[31] Andreeva, Howorth, Chothia, Kulesha and Murzin, "SCOP2 Prototype: A New Approach to Protein Structure Mining". The flexibility of the approach and how that flexibility was achieved are set out on page D312. For the expectation that research would benefit see page D311.

This is standard practice in mathematics. For example, a group is a set with a binary operation on its members, where the set and the operation together satisfy certain conditions.

The method is also available in physics and chemistry. For example, in physics, the electrical resistance of an object is defined as the voltage across it divided by the current flowing through it. In chemistry, a great many technical terms are given precise definitions.[32]

This approach to giving content is practical low down the scale, because tightly-knit theories and clusters of theories are the norm. The resources that are supplied by some basic concepts suffice to give full and useful contents to other concepts. Moreover, the close connections that are forged between concepts, mediated by accepted theories, would make it hard for researchers to adopt alternative contents of concepts. We can therefore expect little disagreement over content.

The use of formal definitions does not however exclude changes of content as disciplines develop. If theories are replaced by other theories or are developed in new ways, concepts that survive such changes may well come to have significantly different contents. To take an extreme example, the physicist's concepts of space and time were transformed out of all recognition by relativity theory, even though the corresponding concepts in everyday life did not change at all. To take another and less extreme example, the discovery of quasicrystals changed the content of the concept of a crystal.[33]

[32] International Union of Pure and Applied Chemistry, *IUPAC Compendium of Chemical Terminology - The Gold Book*.

[33] Lidin, *The Discovery of Quasicrystals: Scientific Background on the Nobel Prize in Chemistry 2011*, page 2.

3.4.3 Vagueness of extension

Concepts have extensions as well as contents. These extensions may be vague. Three consequences are relevant here. The first consequence is that it may be harder than it would otherwise be to find deductive relationships. We shall note this consequence, but we shall not dwell on it because it is not likely to have a significant impact. The second consequence is that it may be more difficult than it would otherwise be to test claimed deductive relationships. The third consequence is that it may be more difficult than it would otherwise be to make full use of such deductive relationships as there are.

We shall say a little about the notion of extension before introducing the notion of vagueness of extension. We shall then consider the prevalence of vagueness of extension and the reasons why it arises, noting that it is more likely to arise in disciplines high up the scale than in disciplines low down the scale. Finally, we shall consider some consequences of the use of concepts with vague extensions.

3.4.3.1 Extension

Candidates to fall within the extension of a concept will be objects that are of interest to researchers. They will include objects that have in fact existed, will in fact exist or exist timelessly, together with hypothetical objects. For example, economists might discuss a hypothetical industry that was dominated by eight firms. That hypothetical industry would be a candidate for membership of the extension of the concept of an oligopoly. The relevant set of hypothetical objects for a discipline may be ill-defined, and it may shift as a discipline develops, but that will not affect our argument.

The term “object” will have a broad sense. It will cover anything that can be picked out, including properties of other objects, transient events and abstract objects. It may for example be a physical object, an event, a historical development, a rate of change in some circumstance, or a mathematical abstraction such as the topological invariance of some given property. Or it may be a set of objects, or the members of a set considered collectively.[34]

3.4.3.2 Vagueness

Some concepts do not have fully determinable extensions. A concept will fall into this category if, given its current content, there are some candidates for membership of the extension such that researchers could not agree as to whether those candidates fell within the extension, even if they were to acquire all of the information about the candidates that would normally be available. We shall say that a concept has a vague extension when the indeterminability is not trivial.

Concepts with vague extensions may perfectly well pick out natural kinds, at least if we agree with Muhammad Ali Khalidi that natural kinds should be regarded as epistemic kinds, the kinds that researchers pick out in the course of their work.[35] A view of natural kinds as epistemic kinds

[34] There are important questions about the logic of concepts that apply to several objects together but not to those objects singly, such as the concept of a football team. Those questions are addressed under the heading of plural logic. But the ways in which those questions may be answered will not affect our argument. For a full treatment of plural logic see Oliver and Smiley, *Plural Logic*.

[35] Khalidi, *Natural Categories and Human Kinds: Classification in the Natural and Social Sciences*, page 43 for epistemic kinds and section 2.4 for what Khalidi calls “fuzzy kinds”.

would comport with our focus on objects and concepts that are of interest to researchers.

There are three reasons why we speak of determinable extension rather than determinate extension. The first reason is that we do not want to regard a concept as having a vague extension merely because researchers have not yet surveyed all the objects or all the types of object to which it might apply, so that they cannot yet catalogue its extension. Even if no catalogue could ever be regarded as definitive, because new objects might come into existence, researchers might be able to conclude, on the basis of the content of the relevant concept and their experience to date, that there would be very few borderline cases. Then they could conclude beyond reasonable doubt that the concept did not have a vague extension. The second reason to speak of determinable extension is that we want to allow researchers the benefit of additional information about specific objects that they do not currently have, but that they could acquire through the normal practice of their discipline. The third reason is that we are concerned with what researchers could discover. If an extension were fully determinate but researchers could not determine it, unavailable information as to the extension would be useless. The consequences for our confidence would then be the same as if there were no such unavailable information.

We restrict ourselves to the current definitions of the terms that correspond to the relevant concepts. A concept's lack of a fully determinable extension is not to be denied by a claim that definitions could be amended in a way that would remove the indeterminability.

The additional information about candidates is limited to what would normally be available. Concepts lack fully determinable extensions if they suffer that lack given the

current practice of the relevant discipline, or perhaps that practice together with the benefit of modest improvements that are likely to be made in the reasonably near future. We impose this restriction because our interest will be in the effects of the lack of fully determinable extensions on the confidence we should have now. We are not to look forward to some ideal future.

Our interest is in concepts that suffer from modest indeterminability of extension, rather than in concepts with hopelessly indeterminable extensions. We should expect agreement within a discipline on the central cases that fall within a concept's extension, and on the central cases that fall outside its extension. Without that much agreement, a concept is likely to be practically useless. The requirement for agreement on central cases is different from a requirement that for most objects that are of the right general nature to be candidates, it should be determinable whether they fall within or outside the extension. A failure to agree in relation to the majority of objects while there was agreement on central cases might not render a concept useless, although it would severely restrict the extent to which the concept could be used.

Certain deficiencies in the ways in which contents of concepts are given may lead to vagueness of extension. We recall that the content of a concept is what one might reasonably conclude about an object if one were told that the concept applied to it. If content is given both very fully and with great precision, that will reduce or eliminate vagueness of extension. But a deficiency in either respect can lead to vagueness. We shall now consider the effects of these two deficiencies in turn. (Both deficiencies may also lead to indeterminacy of content, but that is not our concern here.)

Suppose that the content of a concept is not given fully, in the sense that the content does not touch on all points that researchers might reasonably regard as relevant to the question of whether an object fell within the concept's extension. (This sense of fullness is chosen for our immediate purposes.) For example, political scientists might give the content of the concept of a democracy in terms of voting rights and fairness of elections, thereby allowing specification with precision. But when faced with a particular country, they might also want to consider the extent to which special interest groups had strong influence over government policy, because such influence would work against every citizen's having an equal say.[36] Then even though the official content was stated precisely, vagueness of extension might follow. It might not be enough to amend the content of the concept of a democracy to include a maximum level of influence of special interest groups, because such influences would be so hard to measure that judgement would sometimes be needed to determine whether a given polity qualified as a democracy, so that vagueness of extension would persist. It would also be no answer to say that the problem here identified could not arise, because membership of extension should depend only on expressed content. To pursue the example of democracies, political scientists have in mind certain work for the concept to do. It should group together polities in which public opinion plays a decisive role, but not necessarily from day to day. There is therefore an implicit content of whatever will allow the work to be done. The problem then arises when it is difficult to give full and precise explicit content to match.

[36] We here pick out just a few considerations. In practice, many considerations can be used together. See for example the 60 factors listed in The Economist Intelligence Unit, *Democracy Index 2014: Democracy and its Discontents*, pages 40-49, and the procedure for translating scores into types of polity on pages 37-38.

Now suppose that content is given fully, but not in such a way as to yield a mechanical procedure that would always determine whether the concept applied to a given object. The classic example is that of a bald person. Attributions of baldness depend entirely on the quantity of hair on a person's head. But baldness does not require there to be no hair. Someone may have a few strands of hair and still be bald. How many strands would be too many is however left unstated. So while the content of the concept of baldness covers everything that is relevant to determining membership, and in that sense is given fully, the content is not given with great precision.

Lack of precision is less significant than lack of fullness as a source of vagueness of extension in academic disciplines. Lack of precision has however been the more conspicuous topic in philosophical discussions as to whether vagueness of extension should be located in the nature of the world, so that objects can be within a concept's extension to some degree, in the nature of researchers' access to the world, so that all objects are decisively in or decisively out but researchers cannot always tell which, or in the nature of the languages that are used.[37] That question need not detain us. We also need not be detained by questions as to the proper theory of vague language and the appropriate underlying logic.[38] What will matter for our purposes is that there are some candidates, such that researchers cannot determine whether they are in or out.

[37] For examples of some of the main views in play see Schiffer, "Vague Properties"; Kölbel, "Vagueness as Semantic"; Williamson, *Vagueness*. Smith, *Vagueness and Degrees of Truth*, chapter 2, surveys a range of options and pays particular attention to their logical implications.

[38] An important recent contribution to the discussion is Raffman, *Unruly Words: A Study of Vague Language*. Raffman gives a survey of previous work in section 1.1.

As we noted above, we shall only use the term "concept with a vague extension" when indeterminability of extension is not trivial. What counts as non-trivial will vary from one context to another. It is partly a matter of the proportion of objects that lie in the area of uncertainty, and partly a matter of the extent of effects on the discipline, including effects on the extent to which deductive relationships between propositions can be used. The concept of a concept with a vague extension is itself a concept with a vague extension.

3.4.3.3 The prevalence and sources of vagueness

Concepts in everyday life

We shall start by considering the use of concepts with vague extensions in everyday life. Our concern is with academic disciplines, within which researchers have some freedom to use concepts that are not in everyday use. But a consideration of everyday concepts will serve as a useful introduction, particularly since everyday concepts are used in the social sciences and the humanities.

Human beings regularly use concepts with vague extensions in everyday life, particularly the concepts that characterize people. It can be hard to decide whether to describe someone as kindly, as empathetic or as excitable, even though there are plenty of people who definitely fall within the extensions of those concepts and plenty of others who definitely fall outside their extensions. Evaluative concepts can also have vague extensions. For example, it is not always easy to decide whether a novel should be regarded as profound. We can observe the same phenomenon in relation to some non-evaluative concepts that apply to inanimate objects. It is not always clear whether an object qualifies as

a work of art, in the sense that does not imply special praise. Most paintings clearly qualify. Many everyday artefacts clearly do not qualify. But there are also assemblies of everyday artefacts that are placed in art galleries, and people can debate their status as art and learn from such debate without expecting to reach agreement. Concepts that apply to the natural world can also exhibit vagueness of extension. There are plants such that it is unclear whether they are trees or shrubs: lilac plants are an example.

It is no surprise that concepts used in everyday life have vague extensions. Complete definitions of the corresponding terms have never been set out. Even if a dictionary did try to be prescriptive rather than merely recording usage, definitions would have to be brief in order for it to be practical to use the dictionary. Then they would be too brief to eliminate vagueness.

Another reason why we may expect vagueness of extension arises out of the use of concepts in order to navigate the world. Consider for example the basis on which people make sense of the characters and the conduct of other people – the psycho-social understandings that people use. Such understandings use modest sets of concepts, such as the concepts of ambition, affection, desire, wishful thinking, calculation and decision. These concepts are used to construct explanations of the things that people do: “He works long hours because he is ambitious and he thinks his boss will be impressed if he gets a lot done”; “She goes for a run every day, even in bad weather, because she wants to get fit but has decided that she cannot afford to join a gym”; and so on.

A psycho-social understanding is only usable so long as it is based on a modest number of straightforward concepts, and so long as the concepts cohere well enough to allow them

to be combined to produce explanations of conduct. The limit on the number of concepts means that concepts need to be stretched to make them useful in a wide variety of situations, and it is not practical to make clear in advance quite how far they should be stretched. There would also be a considerable risk that precise definitions would unduly limit the applicability of concepts. Some situations in which the concepts would be needed would fall just outside the boundaries that were set by the definitions.

Concepts in academic disciplines

We should not expect to find concepts with vague extensions in mathematics or fundamental physics. Concepts are embedded in elaborate theories, and the contents of concepts are given in full and with great precision. The types of object that might or might not fall within the extensions of concepts are also limited, being confined to mathematical objects or to fundamental particles, forces and the like. The scope for a concept to be confronted with strange beasts that might fall in some grey area at the boundary of its extension is therefore very limited.

As we move up through the natural sciences, concepts with vague extensions start to be used, even if vagueness is not welcomed. For example, it can be hard to say exactly what should count as an example of a given structural motif in protein molecules.[39] Moving up to whole organisms, there are problems with species. For example, biologists might decide that species were to be defined as interbreeding populations that were reproductively isolated from other populations, and they might do so with a view to ending up with a biologically significant classification of organisms.

[39] Johansson, Zoete, Michielin and Guex, "Defining and Searching for Structural Motifs Using DeepView/Swiss-PdbViewer", pages 2-3.

They might then find that the information they could use in order to fit organisms into such a scheme did not suffice to determine exactly which organisms belonged together in a species. Then to the extent that they had formulated concepts of given species, those concepts would be liable to have vague extensions. Indeed, such difficulties have led to criticism of the idea that the concept of a species should automatically be given a distinctive status in the taxonomic enterprise.[40]

As we move into the social sciences, vagueness of extension becomes obvious. For example, in economics, should a given increase in unemployment be classified as cyclical or structural? Some unemployment is clearly cyclical and some is clearly structural, but sometimes there is no clear

[40] For a survey of the whole area see Kunz, *Do Species Exist? Principles of Taxonomic Classification.* For an account of how, in a modest proportion of cases, the natures of organisms make it hard to use standard methods of classification see Diamond, "Horrible Plant Species". Such difficulties do not suffice to show that the concept of a species should be abandoned, or that species should be regarded as arbitrary constructs. Rieseberg, Wood and Baack, "The Nature of Plant Species", emphasizes that in many cases such difficulties do not block the path to a biologically significant classification. (It should also be noted that there are differences between the issues that arise for plant species and animal species.) Nonetheless, there are grounds to wonder whether classification by species is the best that biologists can do. One view, expressed in the context of a discussion of classification generally, is that instead of wondering what might count as a species, biologists should take a step back and ask what properties should be used to define classes of organisms (Parsons and Wand, "A Question of Class", page 1041).

De Queiroz, "Ernst Mayr and the Modern Concept of Species", covers the significance of the general notion of biological lineage and ways in which that notion may be made precise. But our concern here is with the difficulties that arise once a precise concept of species in general has been chosen, rather than with the fact that there are several competing concepts. The difficulties we have noted here could arise with any precise concept of species in general that could be expected to yield a biologically significant classification of organisms.

verdict and only a broad-brush conclusion is possible.[41] Researchers may however prefer broad-brush conclusions in which they can straightforwardly believe to more specific conclusions in which they could only have something like Stephen Schiffer's vagueness-related partial belief.[42]

Moving on to the humanities, we find the regular use of concepts that are drawn directly from everyday ways of characterizing people and their actions. The author of a biography, for example, tells a human story in human terms. In deciding how to describe the subject, or episodes in the subject's life, the author must use her own judgement. She cannot rely on rules that would make it possible to compute, on each occasion, whether a given adjective would be appropriate.

We should acknowledge that in the social sciences and the humanities, difficulties in deciding whether an object falls within the extension of a concept may reflect deficiencies of data, rather than the nature of the concept. To the extent that deficiencies of data could be remedied in practice

[41] Chen, Kannan, Loungani and Trehan, *New Evidence on Cyclical and Structural Sources of Unemployment*, presents a sophisticated mathematical analysis of examples, but still only presents conclusions in broad-brush terms, using phrases like "substantial impact" (section 7). The difficulty is not that the definitions of the two types of unemployment are imprecise in themselves. A precise definition of the structural unemployment rate is given in section 4.C, and the balance is treated as cyclical. But the information that is ordinarily available does not make it possible to determine exactly which increases in unemployment are structural.

[42] Schiffer, *The Things We Mean*, sections 5.4 to 5.8. Schiffer's account starts from sorites paradoxes, but the notion of vagueness-related partial belief that he develops, which is a recognition that some propositions are sort-of true and sort-of false rather than an expression of uncertainty, could be extended to other claims that were made in reliance on excessive definiteness about the applicability of concepts with vague extensions.

(and not merely in theory), there would not be vagueness of extension as we have defined it. But if that were not possible, there would be vagueness of extension.

It is no response to say that concepts with vague extensions should be discarded, and that other concepts should be used instead. Some of the concepts that show vagueness of extension are too important to be discarded. Classificatory concepts, like concepts of structural motifs and of species, have very useful roles to play in helping researchers to navigate their domains of work and to write up their results for other researchers to use. Discussions of economic policy would be hampered without the identification of different types of unemployment. Many historical accounts would not be anything like they actually are, if they did not explain past events in terms that related pretty directly to life as human beings experienced it. And certain forms of sociological work, such as symbolic interactionism, must by their very nature use concepts that are used in everyday life, or that are only slightly refined versions of those concepts. Then the vagueness of extension of everyday concepts cannot be eliminated.[43]

Finally we may note that while indeterminacy of content may well lead to vagueness of extension, it need not do so. It may be that all candidates for membership of an extension are such that they clearly fall within it, or clearly fall outside it.[44] Researchers are however only likely to be able to reach such a comforting conclusion when they are

[43] This can be seen by reading academic papers such as those in the journal *Symbolic Interaction*. The approach is to understand people's conduct by reference to how they themselves ascribe meanings.

[44] The point is well made in Smith, *An Introduction to Gödel's Theorems*, page 350. It would however be rare in disciplines other than mathematics for researchers to be able to use the kind of squeezing argument that Peter Smith describes on pages 354-355 in order to make extensions fully determinable.

confident that they have surveyed all possible varieties of objects that should be regarded as candidates. The risk that they may not have detected all varieties is likely to be high when the objects are concrete. It may be lower when the objects are abstract, because researchers may be able to work out that they have a comprehensive list of varieties.

3.4.3.4 Consequences of vagueness of extension

The fact that vagueness of extension is more prevalent in higher disciplines is one reason why deductive relationships become less readily available as we go up the scale. Many such relationships rely on the availability of claims that all objects that have one property also have some other property. For example, a claim that all Fs are Gs will support a deductive relationship from “This object is F” to “This object is G”. We must first consider whether such universal claims can gain some support, and then whether they can gain enough support to justify assent to them.

When claims like the claim that all Fs are Gs can only be supported by the enumeration of extensions, vagueness of extension may prevent their gaining any support. But this need not be a widespread consequence of vagueness of extension. A study of the contents of concepts can often be used to support universal claims of this type, so that the enumeration of extensions is not required.

Vagueness of extension can be a serious obstacle to the testing of such universal claims, and hence to working out whether they have enough support to justify assent, even when the claims gain some support from a study of the contents of concepts. For example, a claim that all Fs are Gs can be tested, statistically if not conclusively, by inspecting a sample of objects that are F. If the extension

of the concept F is vague, it will be unclear which objects should be included in the sampling frame. And it is perfectly possible that objects near the boundary of F's extension should differ from objects that fell well within its extension in respects that would make it important to inspect some of the objects near the boundary. Then it might be difficult for a claim that all Fs were Gs to gain enough support to justify assent. Deductive relationships of the form of a relationship from "This object is F" to "This object is G" would then be unavailable, except when the object was such that other support for such a relationship could be found.

Vagueness of extension may also limit the extent to which researchers can make use of some types of deductive relationship to establish facts about objects of study. They may for example wish to make a deduction from a premise that some object is F to the conclusion that it is G, but they may be uncertain whether the object is F because it falls in the grey area at the boundary of F's extension.

It is the availability and the use of relationships that are actually deductive that are most obviously affected by vagueness of extension. Vagueness can make it difficult to propose, to test or to use deductive relationships. But vagueness can have the same effects in the context of relationships that come close to being deductive. The greater the vagueness, the harder it may be to propose relationships by seeing what correlations in the data there are. Vagueness will make it hard to test claimed relationships because it will be hard to determine the proportion of exceptions to the relationship. (To pursue our example, it is only possible to be confident about the proportion of objects that are F but not G if one can be clear whether each given object is F, and it would be dangerous to assume that the proportion among objects near the boundary of F's extension would be the same as the proportion among objects that were well

within F's extension.) Finally, the greater the vagueness of extension, the less scope there will be to use near-deductive relationships to establish facts about objects of study.

3.5 Relationships and assent

We shall now consider how the availability of relationships of different types between propositions can support or hinder the appraisal of claims. We shall first consider the roles that relationships between propositions play in allowing claims to be appraised in certain ways. We shall then see how the strength of relationships and the presentation of arguments for them may affect our confidence.

3.5.1 Ways to appraise claims

When researchers consider whether to assent to a claim, they may consider both which other independently well-supported claims would support the claim, and what would follow from the claim. The availability of these ways to consider claims depends on both the quantity and the strength of the available relationships between propositions. A ready availability of deductive relationships will help a great deal.

We shall first note ways in which the ready availability of deductive relationships can be helpful. We shall then consider the extent to which non-deductive relationships can do the same work.

3.5.1.1 What deductive relationships can do

The ready availability of deductive relationships would afford good prospects for identifying propositions that, individually or in conjunction, would entail a claim that was under consideration. If there were good reason to think that those other propositions were correct, there would be equally good reason to think that the claim was correct. Relationships that supported the drawing of conclusions from theories could be useful, just as much as relationships from statements of particular fact to claims. Relationships could allow accepted theory to predict that some new claim would be correct. That would in turn lend support to the claim. For example, relativity theory entails that time dilation will occur. So when dilation of the predicted extent was first observed, there was theoretical support for the claim that it was a real phenomenon, rather than the observation's having resulted from some defect in the relevant experiments.

The ready availability of deductive relationships would also afford good prospects for using deductive relationships to work out what would follow from a claim, and then seeing whether those consequences held. If any of them did not hold, then the deductive nature of the relationships would require rejection of the claim through the operation of modus tollens. Both deductive relationships that led from claims to the presence or absence of given evidence, and relationships that led from claims to theoretical consequences, could be useful here.

The use of relationships from claims to evidence is obvious. If a claim implies that the value of a measurable variable will fall within some given range, and measurements place it outside that range, the claim must be rejected. At least, it must be rejected unless investigation of the

process of measurement discloses errors, or unless there is some plausible revision of theory that would change the significance of the measurements.

The use of relationships from claims to theoretical consequences is slightly less obvious. It depends on a consideration of whether the theoretical consequences are acceptable. For example, the claim made in 1989 that cold fusion had occurred was inconsistent with accepted theory on how fusion would work and what its by-products would be, so the claim of cold fusion had to be treated very sceptically, for that reason as well as for other reasons.[45]

3.5.1.2 Using non-deductive relationships

We shall now consider the extent to which non-deductive relationships between propositions can do the same work that deductive relationships can do when researchers consider whether to assent to claims.

Non-deductive relationships could help to identify propositions, the correctness of which would support the claim that was under consideration. For example, general principles of human conduct might set out non-deductive relationships which would allow particular propositions to be used to defend a claim that a person's conduct on some occasion was motivated in some particular way – and the general principles might not even be spelt out, if they were obvious enough. An example is provided by an incident in an election.[46] The claim is that an election judge who was disposed to allow a disputed ballot did so partly because

[45] Scaramuzzi, "Ten Years of Cold Fusion: An Eye-Witness Account", section 2.2.

[46] Bensel, *The American Ballot Box in the Mid-Nineteenth Century*, page 112.

of his own political preferences. The supporting particular proposition is that the judge did have relevant political preferences.[47] The general principle in the background, too obvious to be worth stating, is that political conduct is often to be explained by individual preferences rather than by high-minded devotion to fair play. This principle provides a non-deductive relationship from the claim that a specified person had political preferences to the claim that his preferences led him to attempt to influence an election.

Non-deductive relationships could also indicate some probable consequences of a claim that was under consideration. If those consequences were found not to hold, researchers should at least doubt the claim, although they would not have decisive grounds to reject it. The stronger the non-deductive relationships in question, the greater the doubt that should be cast on the claim.

An example of work of this nature is provided by Henry Farrell's review of some variations in business behaviour, with his drawing different consequences from a claim that the explanation lay largely in political cultures and from a claim that it lay largely in institutions, and then coming down in favour of an institutional explanation by considering the evidence. The relevant relationships between claims and their consequences for what evidence should be found are not deductive. They are expressed using phrases like "we are likely to see", "would lead one to predict", and "one would expect".[48]

[47] Bensel does not provide direct evidence that the judge in question was partisan, but is confident that he was. We can extract the message we need by assuming that direct evidence could have been supplied.

[48] Farrell, *The Political Economy of Trust: Institutions, Interests, and Inter-Firm Cooperation in Italy and Germany*, chapter 4. The quoted phrases come from pages 106-108.

3.5.2 Relationships and confidence

We shall now consider the effects on our confidence of the strength of relationships and of the presentation of arguments that support the existence of relationships.

3.5.2.1 The strength of relationships

When the issue is whether a claim is supported by propositions drawn from the evidence and the discipline's corpus, the strength of relationships used in arguments from those other propositions to the claim is very important. Support is transmitted perfectly by deductive relationships, but only imperfectly by weaker relationships. The imperfection rises with the weakness of the relationship. So if our confidence is to be increased by awareness that accepted claims are supported by evidence and by the corpus, we must also consider whether the relationships that transmit support are at least reasonably strong.

When the issue is whether the consequences of a claim are acceptable in the light of the evidence and the discipline's corpus, the position is different. It is not essential for relationships from the claim to its consequences to be strong. Indeed, it is to a claim's credit if even the consequences that only follow from it by virtue of weak relationships are supported by the evidence and fit with the corpus.

3.5.2.2 The presentation of arguments

The presentation of arguments that support the existence of relationships between propositions can affect the signi-

ficance for our confidence of researchers seeing claims as ones that are supported by evidence or by the discipline's corpus. We shall first consider deductive relationships, and then move on to non-deductive relationships. Finally, we shall note the role of diagrams in guiding research.

Deductive relationships

A deductive argument that is put forward to support the existence of a deductive relationship from the argument's premises to its conclusion may be presented as a fully formal proof, with each line either being an established result or following from preceding lines in accordance with the rules of proof. This is the form that may give the greatest assurance. But it is not usually practical to be so formal. Briefer proofs, in which some of the connections between preceding and succeeding lines are obvious to researchers in the discipline and are not spelt out in full, may be a little less satisfactory, but they are standardly accepted even in mathematics, they are the best among the practical options, and they allow almost as much confidence that premises and conclusions are indeed related deductively.[49] Arguments that use other methods, such as diagrams, which make it less obvious how to check a deduction mechanically, are in greater danger of being unsatisfactory.

We should however note that what may be lost as researchers descend the scale of forms of presentation is not the security of any deductive relationships between premises and conclusions that in fact exist. Such relationships are

[49] The discussion of the rhetorical structure of mathematics in Ganesalingam, *The Language of Mathematics: A Linguistic and Philosophical Investigation*, section 2.5, does set out the very strong convention that each sentence should be a logical consequence of what comes before it (page 32), but that is not the same as requiring the formality of presentation that textbooks of logic would prescribe.

perfectly firm, however they are presented. What may be lost is our confidence that researchers will only identify relationships as deductive when they really are deductive. They may be misled into thinking that a diagram or some similarly informal presentation gives a rigorous proof. The danger is that a diagram or other informal presentation may not represent everything that needs to be represented, or it may not represent its subject matter with complete accuracy and precision.

We should not say that diagrams and the like can never establish deductive relationships between premises and conclusions. It is possible to use diagrams to build logical systems for which soundness and completeness results are available, and to give formalized versions of existing diagrammatic methods of reasoning.[50] There are also cases in which diagrams have a claim to be parts of official theory.[51] Moreover, some types of diagram, such as commutative diagrams in category theory, keep mathematicians close enough to the underlying mathematical entities, and do so in a transparent enough way, that there is little or no reason to worry. We should however be cautious about diagrams in general.

This attitude of caution but not exclusion is supported by some comments in the literature. J. E. Littlewood thought that there were occasions when a diagram could be a perfectly adequate proof.[52] On the other hand, it is

[50] For soundness and completeness results in relation to Venn diagrams see Shin, *The Logical Status of Diagrams*, chapters 3 and 4. For the formalization of diagrams in geometry see Miller, *Euclid and His Twentieth Century Rivals: Diagrams in the Logic of Euclidean Geometry*.

[51] Starikova, "Why do Mathematicians Need Different Ways of Presenting Mathematical Objects? The Case of Cayley Graphs".

[52] His example was a fixed point theorem: Littlewood, *Littlewood's Miscellany*, page 55.

all too easy to think that a diagram gives an adequate representation of the objects of study when in fact it does not do so, for example when unbroken lines on paper are mistakenly treated as adequately representing all continuous functions for the purposes of proofs in analysis.[53] Finally, while Brendan Larvor has argued that we should see diagrams as tools to do things which may differ from the things that the sequential expression of propositions set out in mathematical symbols can do, rather than as second-class substitutes for sequences of propositions, it is not at all clear that his line of argument could be developed to the point at which diagrammatic proofs of sophisticated results could be regarded as acceptable alternatives to sequences of propositions.[54]

We may conclude that it is possible for arguments to be presented in ways that will make them appear to be deductive, leading researchers to take it that there are deductive relationships between premises and conclusions, when deductive relationships do not really exist. This may lead to claims being seen as better-supported than they really are, so there is a risk that some claims will come to be accepted when that should not happen. That risk may affect our confidence adversely. And although we have conducted our discussion on this point in the context of mathematics, the risk is not limited to mathematics. It arises in other disciplines in which diagrams are used in the course of making arguments that are supposedly deductive or very nearly so.[55]

[53] Giaquinto, "Epistemology of Visual Thinking in Elementary Real Analysis", section 6.

[54] Larvor, "What Philosophy of Mathematical Practice Can Teach Argumentation Theory About Diagrams and Pictures". Larvor does not claim that an extension to sophisticated results would be possible, and he lays down clear conditions for visual argumentation to qualify as rigorous (in section 13.5).

[55] For an example of such use see Shavitt and Bartlett, *Many-*

The danger should not however greatly concern us. Researchers have a good sense of when informality may lead them mistakenly to regard relationships as deductive. Moreover, if an informal presentation might reasonably be suspected of being misleading, researchers other than its author may well criticize it.

Non-deductive relationships

Moving on to non-deductive relationships, there is no direct analogue of meticulous step-by-step proof, but arguments that purport to establish relationships between premises and conclusions can still be set out with more or less formality. Most notably, visual aids that show how values of variables fluctuate over time or differ from place to place are used to move from evidence to conclusions.[56]

On the one hand, the fact that there is no ideal of a perfect non-deductive relationship means that although there is a risk of exaggerating the degree of support for claims by relying on informal presentations to link evidence and contents of the corpus to those claims, the size of that

Body Methods in Chemistry and Physics: MBPT and Coupled-Cluster Theory, chapters 4 onward. We do not suggest that Shavitt and Bartlett themselves mistakenly regard any non-deductive arguments as deductive.

[56] Examples of papers in which diagrams play conspicuous roles in establishing conclusions, and not merely in expounding them, abound. Two examples of recently published papers are Blundell, Crawford and Jin, "What Can Wages and Employment Tell Us about the UK's Productivity Puzzle?"; Silm and Ahas, "The Temporal Variation of Ethnic Segregation in a City: Evidence from a Mobile Phone Use Dataset". There is a survey of the use of visual representations of data in sociology in Healy and Moody, "Data Visualization in Sociology". The authors argue that visual representations are not merely an optional tool of presentation. They regard their use as central to the practice of the social sciences (page 124).

risk is hard to assess. Moreover, it is less clear than with deductions what might amount to the omission of some consideration that ought not to be omitted, because in a web of non-deductive relationships in which considerations carry a range of weights, instead of being either decisive or irrelevant, it is easy to argue that some considerations might as well be omitted despite the fact that they would carry some small weight.

On the other hand, the fact that relationships are imperfect anyway means that the risk of going astray through the use of informal presentations is not a risk of falling below perfection while still believing that one has attained perfection. It is a risk of being in a position that is not as good as one thought, while being aware that claims do not have perfect support in any case.

Guiding research

There is another role of diagrams that we should consider. This is their role in guiding research, rather than in making connections between evidence, the corpus and new claims.[57] Since the use of diagrams in this role is a relatively uncontrolled process, there is a risk that their use may lead researchers astray, leading them to ask the wrong questions or to focus on some aspects of the evidence while ignoring other important aspects. Whether such risks should affect our confidence will depend on the strictness of the controls that are imposed when claims are appraised. The stricter those controls, the more likely it is that any errors will be discovered.

[57] For examples of this role in guiding research see Sheredos, Burnston, Abrahamsen and Bechtel, "Why Do Biologists Use So Many Diagrams?"; Wheeldon and Åhlberg, *Visualizing Social Science Research: Maps, Methods, and Meaning.*

Chapter 4

Constraints on Claims

4.1 Pervasive claims

In this chapter we shall consider the significance for our confidence of there being pervasive claims that constrain the making of other claims.

Constraints can affect our confidence favourably, because they make the test that new claims should fit in with the existing corpus of the relevant discipline a demanding one. Then incorrect claims may very well fail the test, although it is also possible for correct claims to fail it if there are incorrect claims in the corpus. Pervasive claims can also have a favourable effect on our confidence even when they do not impose particularly severe constraints. The use of pervasive claims in several contexts means that they are likely to have been well-tested, so that their use in any one context can lend some support to other claims that are wholly or partly derived from them. Even if claims are not derived from pervasive claims, they may gain some support from the fact that they comport well with pervasive claims.

We shall start by making some general remarks about pervasive claims. These remarks will occupy the rest of this section. Then we shall consider different disciplines in turn, before ending the chapter with a note on types of quantification.

4.1.1 The identification of pervasive claims

We introduced pervasive claims in section 1.4.2. We shall now recapitulate and elaborate on what we said there.

Pervasive claims are claims within the corpus of a discipline that impose significant constraints on what other claims may be made, across the discipline. (As usual, the discipline in question may be a sub-discipline of a larger discipline.) They may do so most easily by transmitting their influence through deductive relationships between propositions, but they can also transmit their influence through weaker relationships. We gave the following examples in section 1.4.2:

- mathematical axioms;
- claims implicitly made by mathematical definitions, that entities of certain types have certain properties;
- physical laws of wide application, such as the principle of stationary action and the laws of thermodynamics;
- in biochemistry, the claim that proteins are made up of amino acid residues;
- in economics, the Von Neumann-Morgenstern utility theorem (although economists disagree about how useful this theorem is in the study of real economic agents).

Pervasive claims may sit at the foundations of systems of claims, like axioms. But there is no requirement that they should occupy foundational positions.

We should not expect agreement on exactly which claims should count as pervasive. Claims are more or less pervasive. The constraints they impose may be more or less severe. And a claim may be pervasive within a small sub-discipline but not within a larger discipline that encompasses it. Nonetheless, the general idea should be clear enough.

One indicator that a claim is pervasive within a discipline is that if the claim were not accepted, but not generally denied either, a substantial difference would be made to the content of the discipline. There might for example be many other claims that could no longer be seen as enjoying adequate support, so that they could not properly have come to be accepted. Another indicator is that if the claim were generally denied, so that the contradictory claim were accepted, that too would make a substantial difference to the content of the discipline.

The latter indicator would identify more claims as pervasive than the former one. If a claim were neither accepted nor generally denied, other claims might still enjoy enough support from other sources to make assent to them legitimate. But the denial of a claim might very well require changes to many other claims.

We cannot however rely solely on the latter indicator. There are disciplines in which many claims are knit tightly together by deductive relationships – mathematics being the obvious example. In such a discipline, a change from a claim to its contradictory could have very widespread effects through the operation of modus tollens, even if the claim were a minor one. So reliance on the latter indicator would make too many claims pervasive. We must also bear in mind

that the indicators are only indicators. They do not give the full content of the concept of a pervasive claim.

4.1.2 The status of pervasive claims

It is not always clear how to justify pervasive claims, especially not the most fundamental ones. If they need to be accepted in order for other claims to have adequate support, then the pervasive claims cannot themselves derive enough independent support from those other claims. And there may or may not be support available from claims in other disciplines. (For example, the claim in chemistry that only exergonic reactions can take place spontaneously without a supply of energy from outside is supported by the laws of thermodynamics, which belong to physics.)

Fortunately, uncritical assent even to the most fundamental pervasive claims of a discipline is not required. They may need to be accepted in order for the discipline to have its form and general content, but researchers could still debate whether they should assent to given pervasive claims. (Actual debates are rare. We only argue that accepted pervasive claims can be challenged.)

The price of such a debate would be that researchers would at the same time have to debate the form and general content of their discipline. But this price would help to make debate over the claims worthwhile. If researchers tentatively assented to pervasive claims on the basis of some evidential support, and then found that the claims were very fertile, allowing researchers to make connections between accounts of different topics or to advance their discipline, then the researchers could reasonably assent to the claims. If pervasive claims were to be challenged, they might be justified not, or not only, by looking down to

foundations beneath them, by empirical evidence that bore directly on their correctness or by appeal to any supposed self-evidence (a notion of doubtful worth in this context, given that some of the best theories are starkly at variance with the intuitions of those who have not already been acculturated to the relevant disciplines), but by looking up to what could be built on them.[1]

4.1.3 Concepts and pervasive claims

As we saw in section 3.4.2, concepts can have more or less determinate content. The lower we go down the scale of disciplines, the more extensive the use of ways to give content that tend to make it determinate. This matters in relation to pervasive claims.

[1] This is not to say that looking up is always the best approach. It corresponds to the extrinsic justification that is standardly contrasted with intrinsic justification based on self-evidence: Maddy, *Defending the Axioms: On the Philosophical Foundations of Set Theory*, page 47. As Maddy sets out in her context of the axioms of set theory, extrinsic justification is not without difficulties and inadequacies that are quite enough to keep intrinsic justification in play (chapter 5, sections 3 and 4). And there is no reason to think that a move to category-theoretic foundations would change this. (Maddy makes a brief comment on category theory on page 34, footnote 68. For the role of category theory in providing foundations see Lawvere and Rosebrugh, *Sets for Mathematics*.)

For a discussion of the relationship between the self-evidence of axioms and an appreciation of what can be done with them see Shapiro, “We Hold These Truths To Be Self-Evident: But What Do We Mean By That?”.

Finally, the fact that the references in this note have been to axioms of mathematics, which happen to be the most obvious pervasive claims, should not obscure the facts that there are pervasive claims in other disciplines too and that the question of how to justify them is a serious one.

Determinacy of content allows claims to be stable across contexts. If a content is determinate, researchers can be sure that there is no ambiguity of content that could lead to concepts having different contents on different occasions of use. Claims that make use of concepts with determinate contents can therefore be regarded as the same claims in different contexts, and in particular in different parts of a discipline. If the contents of concepts are indeterminate, that benefit may be lost. It will then become doubtful whether claims that appear to be pervasive really are pervasive claims, or are merely members of sets of closely related claims.

This matters when we seek to rely on relationships of support or good fit, rather than constraint, between pervasive claims and other claims, in order to increase our confidence in those other claims. If a claim is directly supported by a pervasive claim, or even if it merely comports well with a pervasive claim, that will be reassuring because the pervasive claim will have been tested in several different contexts. But this benefit can only accrue if it is the same pervasive claim that has been tested in different contexts. If subtly different claims have been tested, the apparently pervasive claim will not have built up a single large balance of credibility. Instead, different claims will have built up separate balances. So indeterminacy of content, as a source of uncertainty about whether an apparently pervasive claim really is a single claim in all contexts, may diminish the extent to which apparently pervasive claims can increase our confidence in such ways.

We should not suppose that indeterminacy of content always has this consequence. It only presents a risk that apparently pervasive claims are not the same claims in all contexts. They might in fact be stable across contexts. Moreover, variants of a claim might be similar enough to

make it proper to regard the variants as versions of a single pervasive claim, so that much of the credibility that each variant acquired from its use in its own context would accrue to all variants. That would help all of the variants to support other claims.

The point does of course apply to the use of non-pervasive claims too. If a claim substantiated in one context is used to support another claim in a different context, it is important to ensure that the former claim is the same claim in both contexts. But the point is particularly likely to be significant in relation to pervasive claims.

4.2 Mathematics

Mathematics has special features. We shall now explore two closely related features: deductive relationships reign supreme, and pervasive claims tightly constrain the making of other claims through the medium of large-scale deductive structures. We shall go on to consider the significance of these features for our confidence. Finally we shall consider the relationship of mathematics to the actual world, and the consequent safety of mathematical claims from the contingencies of the actual world.

4.2.1 Deductive relationships

The ready availability of deductive relationships in mathematics means that it is perfectly possible to require claims to enjoy deductive support, without limiting the discipline. And the norms of mathematics do indeed require

that arguments proceed by way of deduction.[2] This is so even when mathematicians derive claims that are about probabilities, or claims that almost all (but not quite all) objects of some type have some property.[3]

Mathematicians do not continually refer back to axioms in order to set out proofs with the degree of formality that a textbook of logic would prescribe.[4] But we can still think in terms of a great structure of results that is founded on a few axioms, even if we cannot expect absolutely everything to be captured by such a structure. And reverse mathematics, the project of establishing the minimal starting points for various parts of mathematics, shows us that there is scope to specify minimal foundations with some precision.[5]

[2] This is subject to the qualification that, as we noted in section 1.5.2, there are some mathematicians who argue that there is a place for work that does not present rigorous deductions: Jaffe and Quinn, "'Theoretical Mathematics': Toward a Cultural Synthesis of Mathematics and Theoretical Physics". But this is on the understanding that any lack of rigour should be signalled.

[3] An example of a claim about probabilities is the claim that if a random variable is normally distributed, the probability of its taking a value within one standard deviation either side of the mean is approximately 0.683. An example of a claim that almost all objects of a certain type have some property is the claim that almost all graphs constructed in a certain way are isomorphic to the Rado graph. The construction is to start with a countably infinite number of nodes and then draw edges at random, with a fixed probability that is neither 0 nor 1 of an edge being drawn between any given pair of nodes.

[4] This can be verified by reading published papers.

[5] For an outline of reverse mathematics see Simpson, *Subsystems of Second Order Arithmetic*, section 1.9. We must acknowledge that reverse mathematics as generally practised works with subsystems of second-order arithmetic, rather than building everything on the single foundation of set theory, and that this limits its scope to certain parts of mathematics. The current practice of reverse mathematics therefore does not in itself show that everything within the scope of mathematics at some given time could be built up from a base that had a determinable minimum size, but only that this would be a reasonable aspiration (subject to agreement that mathematicians could regard

Such a structure of results would be held together by long chains of deduction that led up from the foundations. It would be the ultimate deductive structure. But we also find that however we arrange mathematical results in a structure, there are more chains than those that run up from foundations to particular results. There are also deductive chains that criss-cross between parts of the structure, and that come to the fore when such links are used to establish significant results. The most famous example is perhaps the connection between elliptic curves and modular forms that Andrew Wiles used to prove Fermat's Last Theorem.[6]

The abundance of deductive relationships makes it easy for there to be pervasive claims that tightly constrain the making of other claims, sometimes across the whole discipline and sometimes in particular sub-disciplines. Some pervasive claims are made explicitly, and take the form of axioms for particular fields of mathematics. But many of them are made implicitly, by defining entities in particular ways. For example, once different types of group have

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everything as built up even if they had not eliminated negation incompleteness). Having said that, the preference for subsystems of second-order arithmetic is motivated not by a desire to leave some work undone, but by the specific nature of the project as it is currently conceived. The aim is to show which additions to a foundation for a part of mathematics would allow the development of which additional parts of mathematics. Set theory is rather too expressive to allow fine discriminations between the different requirements of different parts of mathematics.

We must also acknowledge the existence of reasons to doubt the philosophical credibility of a vision of mathematics as built up entirely from set theory. Reasons for doubt are set out in Ganesalingam, *The Language of Mathematics: A Linguistic and Philosophical Investigation*, chapter 7.

[6] An outline is given in Kleiner, "From Fermat to Wiles: Fermat's Last Theorem Becomes a Theorem". The paper makes the point about links between different parts of mathematics in section 8, on pages 30 and 31.

been defined, a great many results about groups of those types follow. As with axioms, definitions can very often be justified by the scope they give to establish useful and interesting results.[7]

4.2.2 Consequences for our confidence

The ready availability of deductive relationships, the consequent scope to conduct mathematics under a norm that arguments must be deductive, and the scope for there to be large-scale deductive structures that enable pervasive claims to impose very severe constraints on the making of other claims, even when those other claims are far distant from the pervasive claims, can all have favourable effects on our confidence. We shall now review some of the ways in which favourable effects may arise.

[7] For a discussion of ways in which definitions can be justified see Werndl, "The Formulation and Justification of Mathematical Definitions Illustrated By Deterministic Chaos". The proposal we make, that they are justified by their mathematical fertility, has something in common with what Werndl calls natural-world-justification, although our concern is with a definition's use in developing mathematics in itself rather than with understanding the physical world. To the extent that definitions are introduced when the scope to develop an area of mathematics has already been foreseen, our proposal also has much in common with what Werndl calls condition-justification, the use of definitions to capture conditions that are recognized as mathematically valuable.

There are further discussions to be had about the distinction between natural and gerrymandered mathematical entities, and about conclusions as to objectivity that one might draw: Tappenden, "Mathematical Concepts and Definitions"; Tappenden, "Mathematical Concepts: Fruitfulness and Naturalness".

4.2.2.1 Decisions on whether to assent to claims

One consequence of the fact that deductions are expected in mathematics is that when a new claim is offered, it is perfectly clear how mathematicians should appraise it. If it is a purported theorem, mathematicians can check whether its proof is acceptable. If it is a claim that some sequence of formulae constitutes a new proof of some established result, they can check that sequence. Occasionally they may come across something that is too radical to be dealt with by reference to existing theorems or standards of proof, but that is very rare. Most of the time, there is no scope for argument over whether to assent to a claim (although proofs can be long and complex, so it may take a while before a verdict is reached). This, together with the fact that the accepted standards have been so successful, should give us confidence that it is very unlikely that the generality of mathematicians with the appropriate expertise would assent to incorrect claims. (It is worth noting that in mathematics, just as in other disciplines, there is work that is very specialized. It may be that a claim comes to be accepted when most of the mathematicians in the relevant area explicitly assent to it, and most other mathematicians simply take it that the specialists are competent to make that decision.)

The ready availability of deductive relationships can also increase our confidence in another way. That availability means that any particular claim is likely to have several consequences. It will have its place in a deductive structure that surrounds it. If the claim were incorrect, there would be a reasonable prospect that some such consequence would also be incorrect and that this would be detected, even if the incorrectness of the claim were not detected directly.

This is not to say that mathematicians always avoid mistakes. Mistakes have been made, and some have gone undetected for a considerable time.[8] It is therefore likely that some incorrect claims still lurk undetected in the corpus. But so far as we can tell, incorrect claims form only a small proportion of the claims that come to be accepted.

4.2.2.2 Precise definitions

Mathematicians give definitions of terms that are both precise and comprehensive. The definitions convey unambiguous information about the entities to which the terms refer, and they convey all the information there is to be had about those entities (apart from deductive consequences that may or may not have been established yet). The contents of the corresponding concepts are therefore fully determinate. The existing corpus gives considerable assistance in the formulation of definitions that meet this high standard. Many properties, such as the properties of continuity and of being a lattice, are already defined precisely, and these resources can be used when formulating new definitions. And the fact that deduction is the norm makes it comparatively easy to formulate precise and comprehensive definitions, because this fact makes it clear what must be covered by a definition of a term. The definition must include enough to ensure that it will always be clear what can be deduced when the term is used, and it must not include anything else.

This does not mean that existing definitions are always perfect. The original definitions on which calculus relied were not at all rigorous, and they were not replaced by

[8] See for example Grünbaum, "An Enduring Error".

rigorous definitions until the nineteenth century.[9] But such inadequacies are likely to be eliminated in due course.

The facts that precise definitions are the norm and that the resources of the existing corpus are exploited mean that it is easier than it would otherwise be to appraise new claims. Mathematicians are well aware of what they are appraising, and it is easy to bring the existing corpus to bear when making appraisals. This should increase our confidence.

4.2.2.3 Lack of choice

Once mathematicians have formulated appropriate axioms and definitions for an area of mathematics, they typically find that they have no choice as to what claims to make. They ask whether a proposition or its negation holds. They find that only one can be proven to hold, and they do not have a choice as to which one. They find themselves constrained by the pervasive claims that are made by the axioms and the definitions for the area in question, and by axioms and definitions for larger regions of mathematics within which that area sits. As they develop an area of mathematics, they build up a large-scale deductive structure into which any new claim must fit.

This lack of choice is beneficial. It gives us confidence that claims within the mathematical corpus are correct and that their negations are incorrect. Our confidence is increased both because the lack of choice is a sign that claims will only be admitted to the corpus if they fit snugly into it, and because the lack of choice shows that the personal preferences of mathematicians cannot influence decisions on whether to assent to given claims.

[9] Lützen, "The Foundation of Analysis in the 19th Century".

Sometimes there are choices to be made. Mathematicians may find that both a proposition and its negation would individually be consistent with an accepted set of axioms and with the theory that is built on them. They must then split the line of development of the relevant area of mathematics into two new lines. For example, when geometry is axiomatized it becomes clear that one can accept the parallel postulate and develop Euclidean geometry, or reject it and develop other geometries.

This sort of choice should not however give rise to any concern. Mathematicians' personal preferences may determine which line is explored more thoroughly. But the need to split a line of development and the precise reason for that need will have been demonstrated mathematically, so a decision to make the split cannot be attributed to personal preference, and the range of sensible ways to make it will be tightly constrained. Moreover, mathematicians will be fully aware of the split and the need to reflect its impact when stating results. For all of these reasons, the occasional occurrence of choice in the development of some area of mathematics should not lessen our confidence.

4.2.3 Mathematics and the world

Mathematics is an enormously successful instrument for understanding the world. It would therefore be easy to think that mathematics was in some way derived from the world. That would give rise to a serious concern. We might think that mathematical claims should not come to be accepted until they had been tested in the world, and we might fear that such testing could be inadequate. But in fact, we should not have that concern. Mathematicians do not need to subject their claims to empirical tests.

We shall now set out a conception of the relationship of mathematics to the world that explains why we should not have the concern, and why empirical tests are not needed. We shall start with some notes on the effectiveness of mathematics. We shall then set out why this effectiveness is perfectly explicable even if mathematics is not in any way derived from the world. It is explicable because choices stand between mathematics and the world. Finally, we shall note the role of the independence of mathematics from the nature of the world in supporting our confidence.

4.2.3.1 The effectiveness of mathematics

It is tempting to think that mathematics is in some way derived from the world because mathematical analyses of the world are amazingly effective. They help researchers to model the world and make predictions. The results can be very impressive indeed.[10] The temptation is great even though there are qualifications of this rosy picture. One qualification is that not every problem of the sort that one might expect mathematics to help researchers tackle is successfully tackled, and that researchers may (perhaps unconsciously) select the problems that mathematics in its current state can solve, giving a misleadingly good impression of the power of mathematics.[11] A second qualification is that simplification may be necessary in order to make mathematical representation feasible. (We shall return to this point in section 4.3.2.1.) A third qualification is that in order to get mathematics to fit the world and

[10] The effectiveness of mathematics is most famously discussed in Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences".

[11] For this and other concerns see Abbott, "The Reasonable Ineffectiveness of Mathematics".

allow useful deductions to be made, it may be necessary to go beyond what rigorous mathematics would allow.[12] [13]

Even though our impression of the power of mathematics may very well survive consideration of such qualifications, we should not succumb to the temptation to think that mathematics is derived from the world. A stage of choice means we need not think that mathematics need adjust itself in any way to the world.

4.2.3.2 Choices of parts of mathematics

When researchers wish to use mathematics in disciplines that give accounts of the world, whether natural sciences like physics or social sciences like economics, they choose which parts of mathematics to use. For example, when studying spacetime, physicists choose mathematical tools that allow them to incorporate the curvature of spacetime. Economists may choose certain optimization methods rather than others. And so on.[14] The scope to make choices helps to explain the effectiveness of mathematics. Researchers choose the mathematical tools that are appropriate, and choose exactly how to use them. Mathematics itself is rich enough to offer a wide variety of tools from which to

[12] Cartier, "Mathemagics (A Tribute to L. Euler and R. Feynman)", sections 1 and 5, acknowledges that work in physics is sometimes less than fully rigorous.

[13] For arguments that could lead to fairly heavy qualification of the rosy picture as a whole see Davey, *Problems in Applying Mathematics: On the Inferential and Representational Limits of Mathematics in Physics*.

[14] A conception of the process of choice along these lines appears in Hamming, "The Unreasonable Effectiveness of Mathematics", page 89. The conception is also noted in Maddy, *Defending the Axioms: On the Philosophical Foundations of Set Theory*, pages 8-9.

choose.[15]

The stage of choice insulates mathematics from the world. It saves mathematics from being enslaved to contingencies. Mathematics can develop in its own way, regardless of the nature of the world. If some part of mathematics does not appear to reflect the nature of some aspect of the world, that does not create any pressure to change that part of mathematics. It creates pressure to select or develop a more appropriate part of mathematics to model that aspect of the world.

There may be occasions on which the only way to develop some appropriate mathematical tools is to do something radical. One example of an area in which radical approaches have been proposed is that of quantum mechanics. There is a case for adopting a logic that differs from the logics used in most other contexts.[16] But there is no reason why such radical moves should affect our confidence adversely. Indeed, they are particularly likely to attract critical scrutiny within the relevant research communities.

4.2.3.3 Mathematics as a free-standing activity

We have proposed a reconciliation of the effectiveness of mathematics in the world with the independence of

[15] An account of the ability of mathematics to provide appropriate tools to describe and analyse all manner of physical situations, and an explanation of the origin of that ability in the freedom that mathematics has had to develop unconstrained by any requirement to represent the world, are set out in Dieks, "The Flexibility of Mathematics".

[16] For a discussion of how odd quantum logics can be, and a conclusion that they are not so different from ordinary logics that they cannot be called logics, see Pavičić and Megill, "Is Quantum Logic a Logic?".

mathematics from the nature of the world, a reconciliation that relies on identifying a stage of choice. But we can go further. The independence of mathematics facilitates the construction of a discipline with characteristics that allow us to have an especially high level of confidence in accepted claims.

We have already noted the characteristics in question. They are reliance on deductive relationships between propositions, and the use of precise and comprehensive definitions. If mathematics were beholden to the world, it would be hard for mathematicians to identify relationships that they could be confident were deductive. The world has a habit of confronting researchers with exceptions to rules. It would also be hard to formulate precise and comprehensive definitions of terms, because mathematicians would have to make an effort to make the boundaries of the extensions of concepts match boundaries that were to be found in the messy and imperfectly grasped world. The independence of mathematics means that such constraints do not apply to mathematics in itself, although they may be sources of considerable difficulty when the task is to determine how to describe some aspect of the world in mathematical terms.

We should however note a special way in which there can be contact between mathematics and some entities that are neither physical nor creatures of axioms. We must distinguish between axioms that characterize freely invented entities and axioms that attempt to characterize some entities, like the natural numbers, which are independently grasped.[17] The distinction is significant for our purposes

[17] Easwaran, "The Role of Axioms in Mathematics", section 1. For the related distinction between algebraic (definitional) and assertory views of axioms see Shapiro, "We Hold These Truths To Be Self-Evident: But What Do We Mean By That?", pages 175-177. There is another category to note, which Solomon Feferman identifies as foundational axioms: "Why the Programs for New Axioms Need to

because it draws attention to the possibility that there should be some independently grasped entities, and some axioms that were supposed to characterize those entities but that turned out not to do so perfectly. At that point, mathematicians would have a choice. They could change the axioms, they could keep the axioms unchanged and regard them as characterizing some other entities, or they could do both of these and thereby identify two lines of development that could be pursued in parallel. It is important that they would have this choice. Even when axioms are designed to characterize independently grasped entities, the pursuit of the mathematics that is founded on those axioms is not beholden to how those entities should turn out to be. In this way mathematics could avoid being constrained, even if a Platonist view of mathematical objects were correct. It does not however follow that all ways in which this freedom might be exercised in a given case would be useful. Some paths of development might be unproductive. The same is true of the more general freedom that comes from the insulation of mathematics from the world.

We have not by any means given a full explanation of the effectiveness of mathematics, nor have we drawn any ontological conclusions from its effectiveness. We have mentioned Platonism, but we take no position on whether it is correct. Our confidence in accepted mathematical claims can be very high whether or not Platonism is correct. It might be higher if Platonism were incorrect, because we would then have no fear that mathematicians

be Questioned", page 403. These are axioms that apply right across mathematics. The examples Feferman gives are axioms for numbers, sets and functions. While axioms for these entities do look like characterizations of entities that are independently grasped, it could be debated whether the characterization of independently grasped and ubiquitous entities was the only route to axioms' having universal application. While Easwaran refers to Feferman and borrows the name "foundational axioms", he does not explore that particular debate.

were misperceiving independent mathematical objects, but since the philosophical question remains an open one, and since the precise form that any correct Platonism would take is undetermined, it would not currently be fruitful to bring a discussion of Platonism into a consideration of our main question. Having said that, our emphasis on the creative freedom that mathematicians enjoy requires us to acknowledge that there is a substantive question as to whether one metaphysic is as good as another as a background to that freedom. Julian Cole has expressed the view that it is not.[18]

4.3 The natural sciences

When we move on from mathematics to the natural sciences, we find that there is dramatically less scope than in mathematics to identify deductive relationships that could be used to derive large parts of disciplines from foundations alone. The fundamental reason is that contact with the physical world is built into the natural sciences. The contents of disciplines must be appropriate to the world, and observations can falsify theories. The stage of choice that saves mathematics from having to correspond to the world is no longer available.

We shall first expand a little on the notion of the need for claims within the natural sciences to be appropriate to the world. We shall then set out consequences for the nature of the natural sciences. Then we shall set out implications for our confidence.

[18] Cole, "Creativity, Freedom, and Authority: A New Perspective on the Metaphysics of Mathematics". Cole sets out the problem in section 1, and his proposed solution in sections 4 and 5.

4.3.1 Appropriateness to the world

Mathematicians can choose to develop their discipline in whatever ways they like. They are free to identify new entities, and to assign properties to those entities as they wish. They then see what can be deduced about the entities.

In the natural sciences, the concepts used and the claims made must be appropriate to the world. Researchers cannot simply choose the concepts and the pervasive claims that will most easily yield large and elaborate structures of results, and then deduce whatever results they can. Their concepts must be ones that help to marshal and make sense of the data that the world offers. Researchers must therefore pick concepts that will capture the salient features of the world. Their choice of concepts may itself affect the salience of features, but only to a limited extent. It is not permissible to impose an arbitrary theory and then use it to force researchers to view the world in a way that would make the theory appear to work.

The need for claims to be appropriate to the world is reflected in the fact that even when researchers have found an elegant and strongly supported mathematical representation of some phenomenon, they must not simply assent to the consequences that they can draw out purely through mathematical manipulation. The representation might have failed to take account of some factors that would influence outcomes in the world, or there might be some other way in which it was not quite as good as it would appear to be. Researchers should therefore see whether the consequences that follow from mathematical manipulation do in fact arise. They thereby test the adequacy of their mathematical representations to the world.

The requirement to be appropriate to the world can be a very strict constraint (subject to whatever scope there may be for data to leave theories underdetermined, a topic that we shall discuss in section 6.1.1.2). A theory can exploit deductive or near-deductive relationships between propositions to make very specific and quantified predictions, and if the world does not fulfil those predictions the theory must be changed. The corresponding benefit is that if a theory makes predictions and the world does fulfil them, that can give considerable confidence in the theory.

4.3.2 Consequences

We shall now consider some consequences of the need for theories to be appropriate to the world. The consequences we note here centre on the complexity of the world. Theories must either take account of this complexity directly, or adapt themselves in order to allow scientists to represent the world while ignoring some of the complexity. Adaptation is required as soon as we move any great distance away from fundamental physics. Simplification is the most obvious adaptation, and it may require us to re-think the picture of scientists as simply testing theories by taking observations. The complexity itself makes it impossible to see natural sciences that are at some distance from fundamental physics as built up deductively from fundamental physics. The fact that simplification is necessary means that in many natural sciences, exceptionless laws are not available, and that makes it hard to build up extensive deductive structures. Lastly, as we go up the scale, we see the adaptation of bringing high-level concepts into use. The use of those concepts increases the difficulty of establishing deductive relationships. One result of all of these consequences is that deductive structures that would span large parts of

disciplines are not to be expected. There is correspondingly little scope for pervasive claims to impose the severest constraints on the making of claims across large parts of disciplines.

4.3.2.1 The complexity of the world

The complexity of the world means that simplifications must be made. For example, computations of how fluids will flow are normally only practical if a fluid is treated as a continuum rather than as made up of discrete molecules. (Even then, computations are very difficult.) Then the claims that theories make about what should happen in specific cases are not the claims that would be deduced from fundamental theories of physics, which would recognize the existence of the molecules. A theoretical background is by no means irrelevant at this point. It can tell physicists that given the sizes of the molecules, the simplification of assuming a continuum can be a perfectly reasonable one. It can also indicate when such simplifications are likely to mislead so that more sophisticated methods are needed, at the cost of requiring greater computational power.[19]

We may take another example from biochemistry. The reactions that take place reflect the basic rules that govern how atoms and molecules interact, and those rules can be understood in terms of fundamental physics. But modelling cannot in practice start from such a fundamental level, and even when biochemists start at a much higher level,

[19] Wijesinghe and Hadjiconstantinou, “Discussion of Hybrid Atomistic-Continuum Methods for Multiscale Hydrodynamics”, illustrates how the merits of different ways to make computations can be assessed, and how different methods can be combined when that gives the best compromise between exactitude and computational practicality.

simplified models are routinely used. Having said that, we find that as with fluid flow, a theoretical background can give an understanding of the extent to which certain simplifications are acceptable, and of the reasons why they should be acceptable to that extent but no further.[20]

One consequence of the need to find ways to make the conduct of science practical in the face of the complexity of the world is that doubt is cast on a simple picture of scientific procedure as the testing of theories by working out their observational consequences, performing experiments, and comparing the actual observations with the predicted ones. Jody Azzouni has argued that the amount of juggling with simplifications, mathematical shortcuts and the design of experiments that has to go on in order to make science practical invalidates that picture.[21] Agreement with Azzouni's view would reduce our confidence a little bit, because the juggling might let a claim's conflict with evidence go undetected. On the other hand we should note that testing need not proceed simply by taking each claim in turn, working out an observational consequence, and performing an experiment. It is perfectly possible to use a battery of tests to work on a collection of hypotheses and to use the results of early tests to improve subsequent rounds of testing, thereby reducing the rate of error.[22]

A consequence of the complexity that requires acts of simplification is that it is impractical to work out, from the foundations provided by the fundamental theory that physics supplies, exactly what would happen in particular circumstances. The problem is not that fundamental theory

[20] Chen, Niepel and Sorger, "Classic and Contemporary Approaches to Modeling Biochemical Reactions".

[21] Azzouni, *Knowledge and Reference in Empirical Science*, part 1, sections 2 and 3.

[22] Pfeiffer, Rand and Dreber, "Decision-Making in Research Tasks with Sequential Testing".

comprises only general laws, without specific conditions. Even if the initial conditions were fully specified, it would still not be practical to apply the general laws directly in order to work out what would happen. So hypothetical claims of the form "Given these initial conditions, this will be the exact outcome" cannot be deduced from fundamental theory. It might theoretically be possible to deduce such claims, but our concern is with disciplines as they are or could be practised.

This does not break all connections with fundamental theory. As our examples illustrate, fundamental theory can tell researchers a lot about which simplifications work, why they work, and their limitations. Claims that form parts of fundamental theory are still pervasive, setting limits to ways to proceed and sometimes ruling out certain types of claim (for example, claims that would violate the conservation of energy). But their power to constrain other claims is not as great as it would be if there were scope to deduce exact claims from fundamental theory.

The upshot is that within a discipline in which it is necessary to make assumptions that amount to substantially simplifying particle-by-particle descriptions of the world, the claims made cannot be seen as built up deductively from fundamental theory. That route to the construction of large-scale deductive structures is closed. The conditions for a discipline to be practised might be deduced from that theory, but no more than a modest fraction of the content of the discipline could be deduced in that way.

Disciplines do however have their own foundational laws. Nothing we have said so far would rule out deducing large parts of the content of each discipline from its own laws, yielding in each case a deductive structure that would span much of the relevant discipline. But there are two significant

obstacles to doing that. The first obstacle is the difficulty of establishing exceptionless laws. The second obstacle is the use of concepts that have indeterminate contents and vague extensions.

4.3.2.2 The lack of exceptionless laws

In disciplines in which the particle-by-particle detail of the world is overlooked, whether by the making of simplifications or in any other way, laws are prone to exceptions. We set out why exceptions are likely to arise in section 3.4.1.2. The conclusions of deductions that are made by applying exception-prone laws are unreliable because the laws, which act as premises, do not hold universally. Given that the objective of a natural science is to produce a theory that is appropriate to the actual world, it becomes inappropriate to deduce a large proportion of claims by using exception-prone laws. This would be so whether the deduced claims were ones that directly specified what would be observed in given circumstances, or claims at some higher theoretical level.

It might be thought that this would be too harsh a view. After all, if a law was only prone to exceptions on, say, 1 per cent of occasions, the deductive consequences of applying it would only rarely be incorrect. Researchers could recognize and tolerate a low rate of incorrectness. But to the extent that claims about what would be observed in given circumstances could be derived from general laws, they would often need to be derived not from single laws but from combinations of several laws.[23] The potential

[23] This can be seen by looking at scientific papers. It is not unusual to bring a range of theoretical perspectives to bear at once in order to explain observations. A recent example is Tsai and Hu, "Theoretical Analysis on the Kinetic Isotope Effects of Bimolecular Nucleophilic

for there to be an exception to some member or other of a combination would be considerably greater than the potential for there to be an exception to any one specified member. So deduced claims could easily be too unreliable for it to be worth making them. They might happen to be correct even when there was an exception to a member of the relevant combination, but researchers could not rely on that. It would therefore be a hazardous enterprise to deduce the bulk of a discipline from foundational laws.

4.3.2.3 Concepts and deductive relationships

As we saw in sections 3.4.2 and 3.4.3.3, concepts that suffer from indeterminacy of content and vagueness of extension become more significant as we go up the scale of disciplines. These developments make it harder than it would otherwise be to establish and make use of deductive relationships, as we set out in sections 3.4.2 and 3.4.3.4. Large-scale deductive structures, including structures that would be needed to build up large parts of disciplines from foundational claims, become correspondingly hard to establish. The scope for claims to be pervasive, and for claims that are pervasive to impose severe constraints on the making of other claims, is therefore reduced.

Substitution (S_N2) Reactions and Their Temperature Dependence", and in particular section 4. The use of several theoretical perspectives indicates that if derivations of claims about what would be observed were to be based on general laws, several laws would need to be used at once.

4.3.3 Implications for our confidence

If accepted claims are inevitable consequences of fundamental laws or of other pervasive claims that have been very widely tested, that should increase our confidence in those claims. Such deduction would be the ultimate form of constraint imposed by pervasive claims on the making of other claims. But even in the natural sciences it is not possible to deduce everything from fundamental laws.

The general influence of pervasive claims is also reduced. The lack of large-scale deductive structures means that the perfect transmission of influence that would be provided by deductive relationships between propositions is mostly only available over short ranges, while non-deductive relationships only transmit influence in a way that makes the constraints imposed become less severe with distance.

It follows that specific pervasive claims will tend not to constrain the making of claims over wide areas with very great severity. And as we go up the scale of disciplines, there will be fewer claims that can qualify as pervasive at all. These facts will limit any favourable effect on our confidence.

There is also the concern that theories may not run headlong into experimental evidence without the blow being open to being softened by a stage of juggling with simplifications.

4.4 The social sciences and the humanities

4.4.1 Trends as we move up the scale

As we move up the scale of disciplines, deductive relationships between propositions become less and less readily available, as do relationships that are almost as strong. It therefore becomes harder for pervasive claims, whether foundational or not, to impose severe constraints on the making of other claims. The scope for the influence of pervasive claims to give us confidence in other claims is therefore diminished.

This does not mean that relationships between propositions lose their power to give us confidence. Researchers make substantial use of non-deductive relationships. A dense network of such relationships can give us considerable confidence. But the source of confidence would not be the constraining role of pervasive claims.

We shall now consider theoretical variants of disciplines, in which pervasive claims can constrain the making of other claims, and then the use of frameworks to organize claims.

4.4.2 Theoretical variants of disciplines

There is scope for some disciplines to have theoretical variants, in which some of the complexities of the world are ignored. There may then be scope to set out deductive structures that would allow pervasive claims to impose severe constraints on the making of other claims.

Economics provides a leading example. Economists may make assumptions about flows of information, the flexibility of firms and markets, and feasible rates of change in the availability of resources. They may then compute both equilibria and functions that represent the effects of changes to natural conditions or government policies.

Such theoretical variants are very useful, even though the worlds they construct are artificially tidy. They give a starting point for understanding the world. A variant of a discipline that does seek to represent some part of the world as accurately as possible may well be built around a theoretical variant, and may make extensive use of the insights that the theoretical variant affords. Behavioural economics provides an example. This discipline focuses on the ways in which the behaviour of economic agents differs from what has traditionally been assumed by economists, but it can still be seen as built on classical economics.[24]

When researchers use a theoretical variant as a starting point in the analysis of the world, they must however always bear in mind the artificial nature of the theoretical variant. Our confidence in claims about the world can be increased by indications that the use of theoretical variants to represent the world has been subjected to rigorous scrutiny. The absence of such scrutiny would suggest that researchers had been too ready to assume that the world was as tidy as their theoretical models.

[24] Baddeley, *Behavioural Economics and Finance*, pages 8-10. As Baddeley notes, this is not the only view. There are those who see behavioural economics as striking out in a new direction.

4.4.3 The use of frameworks

It is very useful to have a way to organize claims. It helps researchers to survey the currently accepted claims. It also makes it easy to select contexts in which to place new claims, and that is a great help in deciding whether to assent to them. A context will make clear which parts of the existing corpus should carry particular weight when deciding whether to assent to a new claim. Researchers may therefore wish to impose frameworks, in order to allow claims to be organized.

In mathematics and the natural sciences, the appropriate framework is often obvious. Even though, in the natural sciences, there is a shortage of deductive structures that span large parts of given disciplines, there are enough relationships of dependence of parts of disciplines on other parts to make some frameworks of topics much more natural than others. But in the social sciences and the humanities, it is necessary to think harder about choices of frameworks and the consequences of those choices.

Researchers in a historical discipline may use a chronological framework, with periods demarcated in ways that may not have been used at the time but that still help to organize work. In a non-historical discipline (and indeed in a historical one too), researchers may use a framework of recognized topics.

We need to consider the influence of the use of such frameworks on the making and the appraisal of claims. We shall consider chronological frameworks and frameworks of topics separately.

4.4.3.1 Chronological frameworks

It is perfectly normal in a historical discipline to give a framework by picking out stretches of time. Some stretches are defined by little more than dates. These may be highly imprecise dates, as with the division of European history into ancient, mediaeval and modern periods. Other stretches are defined by a combination of dates and significant events or significant features of a period, as with the English Restoration period.

A framework may also be given by listing significant people or events. Musicologists might for example give a chronology of the most significant composers, and use it to give a framework within which other composers could be placed. Art historians might do something similar by giving a chronology of the most significant developments in painting, creating a framework within which individual artists or paintings could be placed.

Chronological frameworks have great psychological appeal. They marshal events, and thereby help people to keep track of what happened and when. But we must ask whether their use may encourage researchers to assent to claims more readily than they should. Researchers might for example be led by a framework to see patterns that might have been seen very differently if the framework had not been adopted. An example, taken from the history of ancient Egypt, is the supposed oscillation between kingdoms with power centralized and intermediate periods with power decentralized in much the same way each time. The usual framework of kingdoms and intermediate periods has in the past encouraged historians to see this pattern, but there are arguments that the pattern is misleadingly tidy.[25]

[25] Van de Mieroop, *A History of Ancient Egypt*, page 20.

Researchers can also misread the past by assuming that some features of a current framework would have had a marked influence on how the people being studied would have perceived their own eras. It is for example natural for those who study English history to pick out the period from 1485 to 1603 as the Tudor period. The very name conjures up images of confident monarchs, fashioning a powerful government at home and dealing ably with threats from abroad, particularly since the two monarchs who first come to mind are Henry VIII and Elizabeth I. But there are arguments that contemporaries did not see themselves as living in a distinctively Tudor age to anywhere near the extent that is seen now. Historians could therefore be led astray if they were to assume, without a study of the detailed evidence, that there was some special degree of loyalty to the ruling family.[26]

Another risk of a framework is that once the start of a period is seen as a turning point, developments within that period may be seen as wholly new, with not enough attention being paid to antecedents. The notion that 1485 was a date of particular significance again provides a convenient illustration. Historians can come to believe that striking political developments in the Tudor period were wholly new, even when they were not.[27]

Researchers should always be aware of such risks, but they do not make a case against the use of frameworks. Researchers simply need to consider whether frameworks influence their views of the past, and then make appropriate additional checks on claims before making or assenting to them. They might for example ask whether they would regard a claim as adequately supported if they were to use

[26] Davies, "Tudor: What's in a Name?", page 36.

[27] Bernard, "Law, Justice, and Governance: New Views on Medieval Constitutionalism", page 332.

a different framework, in which the main events that were used to structure the chronology differed from the events actually used.

4.4.3.2 Frameworks of topics

Use of a framework of topics does not give rise to the same risks as are associated with use of a chronological framework. It is not based on stretches of time, so its structure does not directly lead researchers to see changes. On the other hand, the use of a framework of topics might lead researchers to assent to claims when they should not do so, because the framework might lead them to focus on too narrow a range of considerations. They might confine their attention to other work on the same topic when it would be worth taking a broader view. Fortunately, there are reasons why we should not be greatly concerned at this possibility.

One reason is that there is substantial debate among researchers. Researchers will have different perspectives and will be aware of detailed work on different ranges of topics. If something from another topic is relevant, there is a reasonable prospect that someone will notice and will raise the point.

Another reason is that topics tend to emerge from the pursuit of lines of enquiry that turn out to be fruitful. To the extent that topics emerge from experience of what works, the field of work of the discipline is automatically carved at the joints.[28] A focus on each such topic is then likely to be a sensible focus.

[28] Plato, *Phaedrus*, 265e.

A third reason springs from the paucity of far-reaching chains of deductive or near-deductive relationships between propositions in the social sciences and the humanities. This makes it less likely than it would otherwise be that information from distant parts of the discipline will have great relevance to accounts of a given topic. Researchers need not worry greatly that information about some apparently unrelated topic might render a claim untenable.

4.5 Types of quantification

Quantified accounts are the norm in the natural sciences. They may also appear in the social sciences and the humanities. There is a reasonable correlation between changes in structural features of disciplines as we go up the scale and the decreasing significance of quantification. This is therefore a convenient place to consider different types of quantification.

Quantification is often thought to be particularly reassuring. If claims are expressed in numerical terms, we expect them to be open to being tested with great severity. But we must look more closely. We shall set to one side work in which numbers are written down but are not analysed in any way. And among pieces of work that do involve analysis we shall distinguish two types of quantification, a stronger and a weaker type.

In the natural sciences, accounts often include equations that relate the values of different variables. Given the values of some variables, researchers can work out the values of others. Equations sometimes combine to give a unified theory. This is the stronger type of quantification. It may however be weakened somewhat by any significant role

for free parameters, which may lead to a suspicion that the appearance of a unified theory is only achieved by choosing values for parameters which have no independent justification.[29]

The weaker type of quantification, common in the social sciences and in some types of history but also sometimes used in the natural sciences, is limited to using moving averages, correlations, analyses of variance and other descriptive statistics to establish patterns of change in the values of variables and relationships between the values of different variables, without a unified theory's being constructed.[30]

When an account displays the stronger type of quantification, and in addition the equations form a tightly-knit set and there is no great dependence on values that have been assigned to free parameters without independent justification, the likelihood that researchers will reject incorrect claims will be higher than it would otherwise be. Researchers can tell beyond reasonable dispute what to test and what would count as a claim's failing a test. They can also identify and assess the support for claims that may be provided by the state of the theory as a whole and by neighbouring theories.

[29] Smolin, *The Trouble with Physics: The Rise of String Theory, The Fall of a Science, and What Comes Next*, pages 196-197, makes this criticism of some forerunners of string theory. Smolin argues that string theory itself can only avoid the problem by adopting the grim alternative of allowing a large number of possible solutions. But this debate is by no means over.

[30] An example from the natural sciences is Pingali, Shinbrot, Hammond and Muzzio, "An Observed Correlation Between Flow and Electrical Properties of Pharmaceutical Blends". An example from the social sciences is Brand and Thomas, "Job Displacement among Single Mothers: Effects on Children's Outcomes in Young Adulthood". An example from economic history is Temin, "Price Behavior in Ancient Babylon".

It should still be reasonably straightforward to appraise claims when they are made within accounts that display the stronger type of quantification but the equations are not so tightly knit, or there is dubious dependence on values of free parameters. A loosening of the ties between equations may limit the scope to appraise claims by reference to the state of the theory as a whole or by reference to neighbouring theories, and the assignment of values to free parameters may reduce the extent to which claims can gain credibility from their being set within a unified theory or from empirical confirmation, but these aids to decision-making will not disappear.

Moving on to the weaker type of quantification, there is no unified theory and links to neighbouring theories are likely to be very limited. In the humanities, there may not even be much in the way of neighbouring theories. Researchers can still check claims that report data, and claims that report quantified data are particularly easy to check effectively because numerical discrepancies are less likely than non-numerical ones to be obscured by ambiguity. The claims that can be checked straightforwardly may include reports of trends and other patterns in data. But there may be considerable scope to argue about how claims that state interpretations of data should be tested.

Chapter 5

Explanation

5.1 Explanations and claims

5.1.1 Explanations

Accounts may explain the occurrence of given events, or the existence of given physical objects, or the specific natures of those events or objects. Alternatively, they may explain the occurrence or the nature of all events of some given types, or the existence or the nature of all objects of some given types. They may also explain not facts about separate events or objects within given classes, but statistics that are computed from facts about sets of events or objects. They may also explain the occurrence of abstract mathematical patterns. Finally, they may explain the availability of other explanations, as when some very general laws explain the existence of some less general laws. We shall consider explananda of any of these types to be phenomena in relation to explanations of them.

Explanations will typically combine a range of propositions drawn from the corpus with some pieces of evidence.

For example, a prime minister's concern at the level of support for the main opposition party might be put forward as an explanation of her calling an early general election. Elements of the corpus might include the propositions that an electoral win would give another few years in power, and that politicians are highly motivated to retain power for as long as they can. Pieces of evidence might include the results of opinion polls, and economic data which indicated that some unpopular budgetary measures would have to be taken within a few months.

To take another example, human action on peatlands might be put forward as an explanation of a decline in water quality. Elements of the corpus would include propositions that set out processes which would lead to oxidation in dry conditions, so that there would be oxidized peat that might be flushed into the water supply. Evidence would include the proposition that human action had involved draining the peatlands.[1]

We may abstract a general form from such examples. Some explanans, J, will explain some explanandum, K.

The explanans will usually be a complex of claims. It will be important for us to divide the claims in the explanans into two classes. There are general claims such as laws and principles, and there are claims of particular fact. We shall also speak as if the general claims are all made explicit, even though in practice they are often left unstated.

[1] Anderson,"Loss of Ecosystem Services Provided by Peat Bogs in the UK and Finland", page 4.

The explanandum may also be a complex of several claims, but we shall not need to distinguish between the different claims within it. We shall therefore treat each explanandum as a single claim, the conjunction or other truth function of any separate claims within it.

We shall treat both the explanans and the explanandum as made up of claims, rather than facts or putative facts. We shall also speak of the strength of a connection from an explanans to an explanandum. This will mean the strength of the connection from the conjunction or other appropriate truth function of all the claims in the explanans to the claim in the explanandum. As usual, such connections may be deductive, they may be almost as strong as deductive connections, or they may be weaker. And we shall speak of an explanation's forging such a connection, meaning that the giving of it sets out the connection.

The natural sciences are the disciplines in which there is most scope to draw on laws of nature. That option is not widely available in the social sciences or the humanities. But explanations in the social sciences and the humanities can still have great value. Many examples can be found in a wide range of disciplines. A couple of examples, chosen pretty much at random, are explanations by archaeologists of social changes, and an explanation of John Stuart Mill's thought by reference to the peculiar blend of his utilitarian inheritance, the influence of romanticism and the influence of Harriet Taylor.[2]

In this chapter, we shall first make a few remarks about the types of claim that may come to be accepted when an explanation is endorsed by researchers. We shall then

[2] Cherry, Scarre and Shennan (eds.), *Explaining Social Change: Studies in Honour of Colin Renfrew*; Capaldi, *John Stuart Mill: A Biography*.

discuss the use of different types of law in explanations that use laws but do not use principles. (Principles are general claims that do not qualify as laws.) Then we shall explore two leading types of explanation, causal explanations and explanations that use principles. Finally we shall discuss the availability of explanations of different types in different disciplines, the notions of Erklären and Verstehen, and some special types of explanation.

We shall use the word "explanation" instead of the phrase "explanatory account", because we shall concentrate on the explanatory work that accounts do. And we shall concern ourselves only with explanations of phenomena in the physical world and the human world, although the phenomena that may be explained will include abstract patterns. Mathematics as a discipline in its own right is different. In that discipline, claims simply follow from one another and their following is explanation enough, although there are still questions as to which proofs best show why theorems are correct.

5.1.2 The endorsement of explanations

The path to claims coming to be accepted that will concern us in this chapter passes through the endorsement of explanations. When the generality of researchers endorse an explanation, two consequences will follow.

The first consequence is that the claim that the account is indeed an explanation will come to be accepted. Such a claim will have the form "J explains K". We shall call this the claim to explain of the explanation. We shall treat it as a claim about the explanation, not a claim within the explanation.

The second consequence is that claims in the explanans that play explanatory roles will come to be accepted. This may not happen to all of the claims that play explanatory roles, and there may well be other reasons for researchers to assent to the claims, but at least the claims that do significant explanatory work must gain the assent of the generality of researchers, otherwise they could not endorse the explanation. (We shall regard a claim as playing an explanatory role if it does something to make an explanation work, in the sense that if it were omitted and nothing else were added, the explanation would not be as good.)

5.1.2.1 Claims to explain

We shall read a claim to explain as a claim that there really is a suitable connection from the explanans to the explanandum. A claim to explain is not to be read as a psychological claim, a claim that readers of the relevant explanation would find it satisfying.

We shall take it that claims to explain are correct or incorrect, rather than merely being appropriate or inappropriate. Our decision to treat them as correct or incorrect is supported by the fact that claims to explain are put forward as correct by some researchers and are rejected as incorrect by others.[3] But the sense in which a claim to explain may be correct is different from the sense in which other claims may be correct. Other claims are

[3] Examples of discussions as to whether particular claims to explain, or claims to explain within specific fields, are correct, are Lewellen and Nagel, "The Conditional CAPM Does Not Explain Asset-Pricing Anomalies"; Kraus, "Transparency and Determinacy in Common Law Adjudication: A Philosophical Defense of Explanatory Economic Analysis".

correct if they portray the world in an appropriate way. Claims to explain are correct if the relevant explanations forge suitable connections from explanantia to explananda. The standard of suitability in a discipline will be heavily influenced by the conception that researchers have of what their discipline should achieve, and the content of such a conception must be found in the consensus of the generality of researchers. But we also reserve the right to look at a discipline from the outside, and conclude that its standards for assenting to claims to explain are unacceptably lax.

One way in which disciplines vary concerns the number of non-conflicting explanations of the same phenomenon that may exist without undermining claims to explain that are made on behalf of any of those explanations. Low down the scale, there is likely to be a strong preference for a single explanation of each phenomenon, so that even the presence of two non-conflicting explanations which differed in ways that were more than merely presentational would undermine their claims to explain, or at least the claim to explain of one of them. Higher up the scale, there is less likely to be such a strong preference, and more likely to be a recognition that several explanations may all have their contributions to make, as we remarked in section 2.1.2.1. Then it may be perfectly appropriate to make claims to explain on behalf of several explanations of the same phenomenon at once.

Any requirement to find a single best explanation in order to support a claim to explain should be distinguished from a requirement to find a single best explanation in order to support claims that play explanatory roles – the kind of requirement that makes inference to the best explanation a significant form of argument. We shall now turn to how claims that play explanatory roles may gain support from playing those roles.

5.1.2.2 Claims that play explanatory roles

Claims may gain support from their power to explain phenomena. An example is the claim that there was inflation in the early Universe. The claim can be used in conjunction with other parts of physical theory to explain some otherwise puzzling features of the Universe.[4] Another example is the claim that there is an efficacious connection between the occurrence of moderate levels of ecological disturbance and high levels of biodiversity, a claim that would serve to explain some instances of biodiversity – although this claim remains contentious and is arguably in need of refinement before it can be appraised properly.[5]

This support for claims on the strength of their playing explanatory roles depends on a view that incorrect claims would be unlikely to do anything useful in explanations, and could even disable explanations. But we need to say more than that.

There is an important condition for claims to gain support from their playing explanatory roles. Those explanations of a given phenomenon in which a claim plays an explanatory role need to be better than the explanations in which the claim does not do so, both explanations in which it is explanatorily idle and explanations in which it does not feature at all.

We would need a notion of a good explanation, a notion of a type that would allow us to say that some explanations were better than others, in order to apply this condition. We shall defer that topic to section 8.3.1, as part of our discussion

[4] Lyth and Liddle, *The Primordial Density Perturbation: Cosmology, Inflation and the Origin of Structure*, section 18.2.

[5] Roxburgh, Shea and Wilson, "The Intermediate Disturbance Hypothesis: Patch Dynamics and Mechanisms of Species Coexistence".

of the route to explanations that involves a search for good explanations. For the moment we may work with a general idea that explanations should be of types that would have application in other contexts, so that they do not look ad hoc, and should be well-integrated with the existing corpus. (Not every commonly cited virtue of explanations is highly relevant to the quest to support claims by reference to their explanatory roles. The important thing is that there should be reason to think that the relevant explanations give good representations of the world. The virtue of simplicity is therefore likely to be a secondary one, although not irrelevant.)

The requirement that some explanations be better than others is deliberately left vague. At the strict extreme, it could mean that every explanation of a given phenomenon in which a claim played an explanatory role would need to be better than the best of the explanations in which it did not do so. At the relaxed extreme, it could mean that explanations in which a claim played an explanatory role needed to be, on the whole, better than explanations in which it did not do so. And there would be options in between.

Which versions of the condition are even worth considering will depend on the nature of a claim for which support is sought. A claim of particular fact can easily gain support from playing explanatory roles in several explanations of the same phenomenon, as well as in explanations of different phenomena, so relaxed versions as well as strict versions might be applied. A general claim may well be able to play roles in explanations of several different phenomena. It is less likely to be able to play roles in different explanations of a single phenomenon, because different explanations are often made different precisely by their using different general claims. This is not always so. Some

general claims may crop up in several explanations of the same phenomenon. But when a general claim can only feature in one explanation of a given phenomenon, because its presence in any explanation would force the explanation to be the same or very nearly the same as that one, it may well be that the only version of the condition worth considering is the strictest one, the one that is embodied in inference to the best explanation. (We must however limit ourselves to “may well be” at this point. A general claim in this position might for example gain more support from playing explanatory roles in the second or third best explanations of several phenomena than another, and perhaps conflicting, general claim would gain from playing an explanatory role in the best explanation of only one of the phenomena.)

How strict the condition should be made would depend on the discipline, with greater strictness likely to be appropriate lower down the scale. We should also note that the test is to be applied only in relation to phenomena which have first been chosen in order to garner support for claims. A claim does not lose credibility by its not improving the quality of explanations of other phenomena, unless a claim is so fundamental that it should, if correct, explain a wide range of phenomena.

The reason for the condition is this. If explanations that manage without a claim are just as good as those that make use of it, it is perfectly possible that an explanation that manages without the claim correctly portrays the workings of the world (and perhaps that more than one does so, if there is room for several complementary explanations). Then the claim cannot be regarded as supported by its explanatory role. Of course, even when researchers have not identified good explanations that make no use of a given claim, it is possible that there are such explanations not yet

discovered. This is an aspect of the problem of unconceived alternatives, to which we shall return in section 8.3.3.1. But at least so long as the alternatives are unconceived, there is only a risk that they exist. It may then be reasonable to regard claims that play explanatory roles as supported by their playing those roles, at least for the time being. If good alternatives are conceived, such support is not available.

In this chapter, we shall take it that the condition that explanations in which claims play explanatory roles need to be better than those in which they do not play such roles is satisfied. We shall return to the condition in sections 8.3.2, 8.3.3.1 and 8.3.5.3, in the context of the search for good explanations. At various points in this chapter, we shall set out some additional conditions for claims to derive support from their playing explanatory roles.

Denying support to incorrect claims

Suppose that an incorrect claim played an explanatory role in some explanation. Then there would be another explanation, available to researchers, from which that claim was omitted (with or without its contradictory being included). That alternative might well be just as good, and quite possibly better. So the condition we have set out gives some reassurance that incorrect claims will be denied support. It also gives some reassurance that claims, whether correct or incorrect, that are irrelevant to explanations but that happen to be mentioned in them will be denied support, because the omission of irrelevant claims should make no difference to the quality of an explanation.

We should however seek more reassurance than this. An incorrect claim might greatly enhance the apparent quality of an explanation, by helping to forge a strong connection

from the explanans to the explanandum. A supposed but incorrect law of nature could easily do this. And a whole tissue of incorrect claims could provide an apparently excellent explanation of some phenomenon. In seeking to respond to this concern, we must distinguish between general claims and claims of particular fact.

It would be dangerous to endorse a general claim merely because it played a role in one explanation or in a few explanations. On the other hand there are general claims, such as laws of nature, that are endorsed precisely because they play roles in explanations of phenomena over and over again, where those explanations are better than others which do not use the claims. We can acknowledge that such success should confer support, while not allowing occasional success to confer much support.

Turning to claims of particular fact, we may say that if the general claims in an explanation are well-supported, and some claim of particular fact allows the explanation to be given, that claim thereby derives support. The condition that the general claims need to be well-supported is meant to guard against a claim of particular fact's being given such a useful role because incorrect general claims happen to allow incorrect claims of particular fact to be used to complete a picture.

5.2 Explanation using laws

5.2.1 Different types of general claim

Different types of general claim may be used in explanantia. We shall pick out four significant types: exceptionless laws,

laws with exceptions, statistical laws and principles. In sections 5.2.2, 5.2.3 and 5.2.4 we shall discuss the use of laws of each type in turn, and set out some implications for our confidence. Our concern here is with explanations in which all of the general claims in the explanantia are laws rather than principles. We shall defer the discussion of explanations that use principles to section 5.4, because the issues are somewhat different. But to give a taste of what will come there, principles include general guidance on what may be expected, such as the principle that concerns about economic prospects can lead to social unrest. Such pieces of guidance could not qualify as laws.

When it comes to deciding what should count as a law, we shall give more weight to the practice of disciplines than to philosophical considerations about the reliability of laws and the ability to give theoretical reasons why they should hold. We shall therefore not engage in disputes over the lawhood of general claims that some, but not others, would count as laws.[6] We shall indeed be fairly generous in our concept of a law. We shall not for example exclude a general claim from being a law merely because it would not be possible to spell out some conditions deemed invariant or irrelevant by a ceteris paribus clause, where those conditions would need to be spelt out as part of the general claim in order to make it exceptionless.[7] Nor shall we automatically exclude heteronomic regularities.[8] And

[6] We would not for example have any reason to dispute the view expressed in Dorato, "Mathematical Biology and the Existence of Biological Laws", that various biological claims are perfectly good laws, even though others, pursuing other arguments, might dispute that view.

[7] Inability to spell out the details of ceteris paribus clauses has been argued to mean that what appear to be non-strict laws in biology are not really laws at all: Rosenberg, "How is Biological Explanation Possible?", section 2.

[8] Compare the arguments in Henderson, *Interpretation and*

we need not be concerned that general claims may only be correct for reasons that are accidental. For example, we need not look down on general claims that depend on developments in evolution which might well not have occurred.[9] But it is important that the accidents are in the past, so that they are no longer open to being undone, at least not without a course of events that researchers can be confident is very unlikely to occur. We shall not count accidental regularities as laws when the regularities depend on a continuing pattern of accidents that could easily change, so that the regularities could easily cease to hold.

We shall also speak of laws as applying even in situations in which the results are not precisely as the laws would predict, for example because of disturbing factors that the laws are perfectly well understood to disregard (such as the effect of air resistance, disregarded by the normal use of the law of gravity to compute how long stones dropped near the surface of the Earth will take to reach the ground). We shall not equate application with the strict sense of instantiation that Andreas Hüttemann gives.[10]

Our division of laws into three classes is rough and ready. We shall say something about the types of law that would fall under our three headings as we discuss the different classes. But we do not need to draw perfectly sharp boundaries between the classes.

Explanation in the Human Sciences, section 8.3.

[9] See the discussion in Lange, "Laws and Theories".

[10] Hüttemann, *What's Wrong With Microphysicalism?*, page 18. For Hüttemann, a law is only instantiated when things happen exactly as the law says. But as he notes on the same page, failure to be instantiated in his strict sense does not prevent a law of nature's being explanatory.

We do however need a reasonable sense of where to locate the boundary between laws and principles. Where it is possible to estimate the proportion of cases in which a general claim will fail, and that proportion is reasonably low, the general claim will be a law. But if the proportion cannot be estimated or is high, we will have a principle. (The words “reasonably low” may be interpreted by reference to the attitudes of researchers.) This should give us a sense of the boundary even though it will leave some general claims in a grey area, not determining whether they are laws or principles.

5.2.2 Exceptionless laws

5.2.2.1 The strength of connections

In some explanations in physics and chemistry, the general claims in the explanantia are all laws that are believed to be exceptionless. If in addition an explanation is considered to be good by the standards of the discipline, it is natural for outsiders to agree that it is good. There are however several different points to consider.

If the general claims in an explanans are considered to be exceptionless and researchers regard the explanation as good, it is unlikely that all of the claims of particular fact in the explanans could be correct while the claim in the explanandum was incorrect. It is only unlikely, not impossible. The laws cited might be incorrect, or they might have obscure exceptions that had not been noticed. Another risk is that the claims of particular fact in the explanans might not quite match the conditions for the laws to apply. Then it would be possible for both the laws and the claims of particular fact to be correct, while they did not hang

together in such a way as to ensure the correctness of the claim in the explanandum. But leaving such possibilities aside, we can say that a strong connection will have been forged from the explanans to the explanandum. This should increase our confidence in claims to explain made on behalf of such explanations, because the existence of a strong connection is an indication (although not a proof) that an explanation reflects the workings of the world, rather than tracking some side-effects of those workings. For the same reason, strong connections should increase our confidence in claims that do explanatory work. When a claim does explanatory work and there is reason to think that the relevant explanation reflects the workings of the world, those two facts combine to suggest (although not to prove) that the claim is correct.

We have so far considered how the correctness of all of the claims in an explanans may suffice for the correctness of the claim in the explanandum. We should also consider necessity. What are the prospects for saying that a claim in an explanandum would only be correct if all of the claims in the explanans were correct, or alternatively if all members of some specified subset were correct? (We assume for the moment that the explanans is a conjunction of claims. Adjustments could be made to allow for other truth functions.)

Sufficiency does not guarantee necessity. Even if all of the general claims in an explanans are exceptionless laws, it is perfectly possible that the claim in the explanandum would have been correct while some of the claims in the explanans were incorrect. We can however say that if the discipline as a whole is one in which exceptionless laws are the norm, that should give a reasonable prospect of being able to establish that the correctness of certain claims in the explanans was necessary for the correctness of the claim made by the

explanandum, assuming the correctness of the other claims in the explanans. It should do so because when features of the world of the type that a discipline picks out are related to one another by rigid laws, there is a reasonable prospect that changes in features mentioned in an explanans would require changes in features mentioned in an explanandum.

If necessity is established, either for the whole of an explanans or for selected claims within it, that should be reassuring. It is, like sufficiency, a sign that an explanation reflects the workings of the world rather than tracking some side-effects of those workings. The establishment of necessity may therefore increase our confidence in claims to explain. It may also increase our confidence in claims in an explanans where their correctness appears to be necessary for the correctness of the claim in the corresponding explanandum. If their correctness really is necessary and in addition the claim in the explanandum is correct, then the claims in the explanans must also be correct.

These benefits of necessity do not depend on necessity's showing that a given explanation is the only explanation. Necessity would not show that, even when combined with sufficiency, because there might be other ways to characterize the workings of the world, for example if deeper laws were identified, or to characterize states of the world in which all of the claims in the explanans were correct. But the existence of alternative characterizations would not put in doubt a claim to explain made on behalf of an explanation that did not use those alternatives.

There is also a risk that researchers will use laws that are incorrect, although their incorrectness has not yet been exposed. But given that our primary concern is with what confidence we should have in the members of general classes of claims, rather than with the confidence we may have in

individual claims, this risk would not debar us from allowing ourselves a reasonable level of confidence for the time being.

5.2.2.2 Contrastive explanation

An explanation of some phenomenon may show why it rather than something else occurred. That will make the explanation contrastive. Alternatively, an explanation may explain the phenomenon without explaining why it rather than something else occurred, in which case it will be non-contrastive.

If all of the general claims in an explanans are exceptionless laws, the explanation has a reasonable prospect of being contrastive. In particular, if the correctness of all of the claims in the explanans would ensure the correctness of the claim in the explanandum, the explanation is automatically contrastive because any occurrence inconsistent with the claim in the explanandum is excluded.

There is reason to think more highly of contrastive explanations than of non-contrastive ones. If researchers show why one thing happened rather than another, that indicates (although it does not demonstrate) that they have got to grips with the workings of the world. The indication is stronger than it would have been if they had only offered a non-contrastive explanation. A given non-contrastive explanation may happen to be better than a given contrastive explanation of the same phenomenon, if for example the non-contrastive one explains more detail or makes more connections with the existing corpus, but as a general rule, contrastive explanations are the better type.

The general superiority of contrastive explanations means that if explanations are contrastive, that fact may in itself

increase our confidence in the corresponding claims to explain.

The fact that possession of a contrastive explanation indicates that researchers have got to grips with the workings of the world also means that when claims play explanatory roles, our confidence in those claims may be increased more by their playing such roles in contrastive explanations than by their playing such roles in non-contrastive explanations.

5.2.2.3 Support for general claims

One requirement for confidence in claims to explain, and in claims of particular fact that play explanatory roles, is that the general claims in the relevant explanantia should themselves be well-supported.

Strong support is particularly likely to be available when the general claims are exceptionless laws. This is because exceptionless laws are relatively easy to test. If the conditions in the antecedent of a law can be created, the result set out by the consequent does not follow, and possibilities such as experimental error have been ruled out, the law must be discarded. (On the other hand, conclusive support for the exceptionless nature of laws is not available, because there is always the risk that some exception will in due course be observed.) It can be argued that no law can be subjected to an absolutely decisive test, because there may always be some other option than that of discarding the law. But as we shall note in section 6.1.1.2, there are reasons to think that such arguments should not trouble us greatly.

5.2.2.4 Embedding in theories

Exceptionless laws tend to be relatively easy to embed in theories, with the elements in each theory knit tightly together, because the laws set out relationships between propositions that are deductive or very nearly so.

Explanations that use general claims which are embedded in theories may well be regarded as more satisfactory than those that use general claims which are not so embedded. The reason is this. While the use of general claims that are not so embedded may answer an immediate question as to why some phenomenon occurred in given circumstances, explanation is likely to stop there. If on the other hand general claims are embedded in a whole theory, researchers are likely to be able to pursue their enquiries further and ask why the general claims hold. And the more tightly knit and wide ranging the theory, the further it is likely to be possible to pursue explanations. Everyone can recognize this benefit, not only those who subscribe to a unificationist model of explanation and who praise tightly-knit and wide-ranging theories for that reason.[11]

In a discipline in which embedding in tightly-knit theories is the norm, a claim to explain can be judged partly by whether the general claims used can be embedded. If the

[11] For that model see Kitcher, "Explanatory Unification and the Causal Structure of the World", section 4. The connection between unification and explanation has however been challenged, for example in Gijsbers, "Why Unification Is Neither Necessary Nor Sufficient for Explanation", and in Morrison, *Unifying Scientific Theories: Physical Concepts and Mathematical Structures*, especially sections 2.5, 4.6, 6.4 and 7.3. It is also important to be aware of the type of unification that is achieved in any specific case, and whether it implies reduction, since reduction would look particularly satisfactory. For varieties of unification in physics and the connection with reduction see Morrison, "Unification in Physics".

general claims cannot be embedded, that may lead to some concerns about the claim to explain. It may be feared that the explanation does not really identify the workings of the world as they are understood within the relevant discipline. If on the other hand researchers only assent to claims to explain when the general claims used can be embedded, that should increase our confidence in claims to explain. In disciplines where embedding in tightly-knit theories is not the norm, it would be unreasonable to subject a claim to explain to such a test. This does not however mean that we should lack confidence in claims to explain. Other tests are available. For example, checks that the concepts used in the general claims in an explanans are given the same contents as when they are used elsewhere, without any subtle modifications, may give reassurance that there has been no ad hoc adjustment in order to allow an explanation to be given.

When we turn to general claims to which researchers assent at least partly because they play explanatory roles, similar points may be made. In some disciplines, general claims are expected to be open to being embedded in tightly-knit theories. A check to see whether claims can be embedded gives a control over whether researchers should assent to them. The availability of such a test can increase our confidence in such claims. But this does not mean we should lack confidence when the discipline is one in which embedding is not to be expected, so that the test is not available. Other checks may be enough to give confidence.

5.2.3 Laws with exceptions

Some laws have exceptions. If exceptions to a law can be identified they can be listed as conditions under which

the law does not apply, giving an amended law that does not have exceptions. But the result of doing so may be an impractically unwieldy law. There may also be unpredictable exceptions. We must therefore consider what to make of explanations in which some of the general claims in the explanantia are laws with exceptions.

5.2.3.1 The strength of connections

If some of the general claims in an explanans are laws with exceptions, the explanation will not forge as strong a connection from the explanans to the explanandum as it would if all of the general claims were exceptionless laws. If exceptions occur in circumstances of numerous types, there is a good prospect that there will be many ways in which all of the claims in the explanans could be correct while the claim in the explanandum was incorrect. If exceptions occur often, there is a good prospect that in a substantial proportion of actual or hypothetical cases, all of the claims in the explanans would be correct while the claim in the explanandum was incorrect. Either shortcoming would substantially weaken the connection from explanans to explanandum.

If claims to explain individual occurrences of the phenomenon described in an explanandum gained assent despite such shortcomings, we would be right to have less confidence in those claims than we would have had in the absence of those shortcomings. Having said that, a link from explanans to explanandum could be strengthened on a given occasion if the types of circumstance in which the claims in the explanans could be correct while the claim in the explanandum was incorrect, the exception-types, were identified. All that would be needed would be to explain why the instant case was not an example of an exception-

type. This could increase confidence in the corresponding claim to explain on the given occasion, although not more generally, without any need to improve the quality of the general claims. Even showing only that the instant case was not an example of any exception-type, without showing why, might be enough if exceptions were rare. But an explanation of why it was not such an example would be needed if exceptions to the relevant laws were common.

The position is less clear with claims that play explanatory roles. If a claim was used in a single explanation that was used to explain several different occurrences of a phenomenon, that would be a good sign even if the explanation forged a relatively weak connection from its explanans to its explanandum. It would also be a good sign if a claim played explanatory roles in several different explanations, because the successful use of the claim in a range of different circumstances would indicate its correctness. But even then, its correctness would not be demonstrated.

Finally, connections in the other direction, connections which would argue that some or all of the claims in an explanans needed to be correct in order for the claim in the explanandum to be correct, would not have to be made harder to establish by the use of laws with exceptions in the explanans. And such connections could, as outlined in section 5.2.2.1, increase confidence in claims to explain and in claims that played explanatory roles.

5.2.3.2 Contrastive explanation

When it is not possible to forge connections that would make the correctness of all claims in explanantia suffice for the correctness of the claims in the corresponding

explananda, explanations cannot be perfectly contrastive because they cannot show that the claims in the explananda had to be correct in the circumstances, so that incompatible claims had to be incorrect. This does not however mean that the reassurance that can come from the contrastive nature of explanations would be unavailable. Explanations might still show it to be very likely that the claims in their explananda should be correct rather than alternative claims. That would be some indication that the explanations got to grips with the workings of the world.

5.2.3.3 Support for general claims

When a general claim has exceptions in a few identifiable types of circumstance, and the exceptions it has do not occur often, it should be reasonably easy to subject the claim to severe tests. All that would be needed would be to test the general claim several times under each of several different conditions. But as the types of circumstance and the frequencies of exceptions increase as we go up the scale of disciplines, it becomes harder to gain the reassurance that would come from general claims in explanantia having been tested severely. Moreover, the reassurance that may be gained will be limited by the extent to which the types of circumstance that would give rise to exceptions, and the frequencies of exceptions, are unknown.

5.2.3.4 Embedding in the corpus

Laws with exceptions are harder to assemble in tightly-knit theories than exceptionless laws, simply because they do not set out deductive relationships between antecedents and consequents. Embedding one such law among exceptionless

laws might be feasible, but a discipline that makes use of laws with exceptions typically makes extensive use of them, so the materials to create tightly-knit theories that could receive individual laws are not available. But even when embedding in tightly-knit theories is not possible, laws with exceptions can still be more or less well-embedded in the relevant corpus, in the sense that they can have more or fewer, and stronger or weaker, connections with other general claims in that corpus.

The instruments of the embedding of general claims in the relevant corpus are not limited to the direct relationships between antecedents and consequents that are set out in laws. Another instrument is the use of relatively thick concepts, that is, concepts with substantial content. In physics, a concept like that of electric charge is a thin one. It means little on its own, and it gets its significance from its being used in theories. An explanation that merely cited the electric charge on some body would be hardly any explanation at all, were it not for those theories. In physiology, a concept such as that of the basal metabolic rate is a thick one. Its content allows implicit reference to an extensive background, so that general claims which use the concept, such as Kleiber's law, can be regarded as well-embedded in the corpus of physiology.[12]

This kind of embedding in the corpus is not as good a source of confidence in general claims as embedding in tightly-knit theories, but it provides some reassurance. Its absence,

[12] The way in which content allows contact to be made with an extensive background is illustrated by Ballesteros, Martínez, Moya and Luque, "Energy Balance and the Origin of Kleiber's Law". The dangers that may arise from not being able to embed general claims in tightly-knit theories are illustrated by controversy over the status of Kleiber's law: see Hulbert, "A Sceptics View: 'Kleiber's Law' or the '3/4 Rule' is neither a Law nor a Rule but Rather an Empirical Approximation".

when a general claim seemed to bear little relationship to the rest of the corpus of the discipline, would certainly be a cause for concern, casting doubt on the acceptability of explanations that relied on the general claim in question.

5.2.3.5 Ceteris paribus clauses

Ceteris paribus clauses may be used to secure laws against exceptions that are unknown, or that are not spelt out because doing so would render the laws too cumbersome. Such clauses can be very useful in allowing idealizations to be constructed. They allow researchers to exclude from consideration the exceptions that would prevent them from asserting the existence of deductive relationships or other strong relationships between propositions. But that benefit is limited to idealizations. When researchers turn to the world, they cannot be sure that all other things will be equal. They have no right to exclude exceptional circumstances.[13]

When we consider claims to explain, explanations that use laws with ceteris paribus clauses have their limitations. Researchers may be unable to be sure that exceptional

[13] The exclusion of exceptional circumstances is not the only thing that ceteris paribus clauses can do. Building on that function, they can also play a role in allowing researchers in a given domain to formulate and use laws that would be parts of an ideal best system of laws if the ceteris paribus clauses were replaced by detailed specifications of exceptions. A system of laws with ceteris paribus clauses that researchers in fact use may then be a reasonable approximation to an ideal best system: Schrenk, *The Metaphysics of Ceteris Paribus Laws*, section 3.3.4. The notion that scientists pursue the best system in a given domain in the way that the standard best-systems approach to laws suggests is open to criticism, as for example in Roberts, *The Law-Governed Universe*, section 1.3. But we can still see a role for ceteris paribus clauses in facilitating searches for systems that would pass muster.

circumstances which would render some laws inapplicable do not arise in specific cases. To that extent, they should hesitate to claim that explanations are good ones in specific cases. If they do not habitually stop to consider whether specific cases are ones in which the terms of ceteris paribus clauses may be violated, the likelihood that incorrect claims to explain will come to be accepted will be higher than if they do habitually stop to consider that risk. Having said that, a claim to explain which only stated that the explanation usually explained phenomena of the appropriate general type could be perfectly well-supported. Such a claim to explain could be supported by reasons to think that the exceptions to laws which were ruled out of consideration by ceteris paribus clauses would be rare. And in specific cases in which researchers were sure that no relevant law was rendered inapplicable by special circumstances, the rarity of exceptions could excuse them from explaining why exceptions had not arisen. (If however exceptions were reasonably common, researchers would not be so excused. An explanation of a specific occurrence when researchers were sure that circumstances did not render any relevant law inapplicable would then be deficient if it did not set out why no exception arose in that instance.)

Turning to claims that play explanatory roles, we may say something similar. When an explanation uses laws with ceteris paribus clauses, a claim that plays an explanatory role will only be able to derive support from the apparently successful use of the explanation on a specific occasion so long as the explanation as a whole is not rendered idle by exceptional circumstances. If however there was good reason to think that such exceptional circumstances were rare, the claim could derive support from the apparently successful use of the explanation on many occasions, so that a good proportion of them would be occasions on which the explanation was not idle, even if researchers could not tell

which specific occasions were ones on which the explanation was not idle.

5.2.3.6 The combination of laws

We should not assume that explanations in which all of the general claims in the explanantia are laws that are either exceptionless or close to exceptionless will automatically forge strong connections from explanantia to explananda. So we should not simply inspect those general claims, assume the existence of strong connections, and have the corresponding confidence in claims to explain.

Explanations may fail to set out strong connections because when laws are combined, there may not be a single fully determinate, or close to determinate, resultant law that would govern concrete situations. Jan Faye has noted that we should not assume there are higher-order laws that determine how laws interact.[14]

Even if there would theoretically be a single determinate resultant law, it might be prone to exceptions that were of too many types or too hard to identify for it to be feasible to catalogue them. This could very well happen if the different laws involved identified causes and effects of a range of different types. The scope for variation in a set of facts of a range of types would be greater than the scope for variation in the single type of fact with which single laws often deal. Some odd combination of facts could easily lead to an outcome that could not have been predicted without an impractically complex analysis of microphysical detail. And the risk of this happening would be greater than the risk of some odd outcome in relation to a single law.

[14] Faye, “How Nature Makes Sense”, pages 92-93.

Some good examples come from engineering, where machines and other equipment are carefully designed, taking full account of all relevant laws, but unpredictable failures still occur because of odd combinations of facts of various types, such as facts about vibration, temperature and moisture levels.[15] So if some machine did not fail, and the explanans for that happy fact merely stated that levels of vibration, temperature, moisture and so on were within the limits for which the machine had been designed, there would not be a connection of the greatest possible strength to the explanandum, the absence of failure. The machine could have failed despite the correctness of all of the claims in the explanans. (People do not normally seek explanations when things do not go wrong, and the example may therefore seem a strange one. But it would not seem so strange if it were changed to be about the failure rate among 1,000 machines. If that failure rate were zero, the zero rate would not be fully explained by the fact that levels of vibration, temperature, moisture and so on had all been kept within appropriate limits, because one would expect odd combinations of facts to have led to a few failures.)

5.2.4 Statistical laws

Some laws are statistical, rather than relating to single occasions of measurement. There may or may not be exceptions to such laws. Considerations that relate to exceptionless laws and to laws with exceptions may therefore both be

[15] Examples may be found in the journal *Case Studies in Engineering Failure Analysis*. Two nice examples from different areas of engineering are Geary, “Analysis of a Corrosion Under Insulation Failure in a Carbon Steel Refinery Hydrocarbon Line”; Haghshenas and Klassen, “Analysis of Cracks Generated in the Spinning-Mandrel Teeth”.

relevant. Here we shall concentrate on considerations that are specific to statistical laws.

Laws may be statistical because of fundamental facts about the world or about the nature of researchers' contact with the world – a view that is appropriate to some laws within fundamental physics.[16]

Laws may also be statistical because a discipline describes the world in a relatively high-level way, overlooking a great deal of the microphysical detail that would be needed to say what would happen on specific occasions. An example is provided by the Gutenberg-Richter law. For a given region of the Earth, this law sets out a mathematical relationship between the magnitudes of earthquakes and their frequency, with low-magnitude earthquakes being more frequent than high-magnitude ones. The law only gives a distribution for each region. It does not account for individual earthquakes.[17]

[16] The words "researchers' contact with the world" reflect the fact that the statistical nature of the laws in question can be attributed to acts of measurement, rather than to states of the world. In quantum mechanics in particular, states of the world and measurements of states do not have the straightforward relationships to one another that they have in classical mechanics: Susskind and Friedman, *Quantum Mechanics: The Theoretical Minimum*, page 96.

[17] We may add that the law is not quite right for some regions. While there is scope to identify possible reasons for the discrepancies, it can be very difficult to determine the actual reasons: Sue, Grasso, Lahaie and Amitrano, "Mechanical Behavior of Western Alpine Structures Inferred From Statistical Analysis of Seismicity". Thus there would be scope for a higher-level statistical distribution of numbers of regions as more or less closely compliant with the law, although the interest of such a distribution would depend on there being a large enough population of regions. When considering statistical laws, it is important to distinguish the different levels at which distributions may arise.

Finally, researchers may formulate statistical laws because sample and population statistics, as opposed to information about individuals, are of interest in their own right. Statistical laws in either of the first two categories may very well fall into this category too. Means, variances, shapes of distributions and statistical relationships between other statistics may be of considerable importance, but by their nature they can only be computed for groups of individuals. An example is provided by Zipf's law on the relative frequencies of words in large collections of texts drawn from given languages.

5.2.4.1 The strength of connections

Statistical laws may very well feature in explanantia. No special issues need arise when statistical laws are used to explain other statistical laws. The connections from explanantia to explananda may be just as strong as when all the laws involved are exceptionless, because the statistical laws in the explanantia will act as claims of particular fact rather than as general claims. But we do need to ask questions when statistical laws are used as general claims to explain specific phenomena. There are two types of specific phenomenon that researchers might seek to explain by using statistical laws.

The first type is that of some value of a statistic computed for a large set of individual objects or events falling within a range of values that the statistical laws identify as likely. (A range is likely if the probability of an occurrent value's lying somewhere within the range is reasonably high. There may not be any particular value within the range such that there is a high probability of an occurrent value's equalling it.) An explanation that uses statistical laws can show how it is very likely that a large set's statistic will lie within a given range.

The narrower the range that the laws identify as likely, and the higher the likelihood, the more ready researchers will be to assent to the corresponding claim to explain. Unfortunately, there is a trade-off: higher likelihoods are associated with wider ranges, and therefore with vaguer and less interesting explananda. If an explanandum is too uninteresting, researchers may well be reluctant to assent to the corresponding claim to explain, not because of low likelihood but because nothing worthwhile has been achieved. A claim to explain would give a misleading impression of achievement.

The second type of phenomenon is that of some value of a statistic computed for a small set of objects or events, or some value of a variable for a single object or event, falling within a range that is identified as likely. One difficulty with such explanations is that ranges of values with acceptably high likelihoods will usually be wide. Any explanation is therefore likely to have an uninteresting explanandum.

We shall confine our attention to explanations where sets are large enough to make likely ranges of values narrow enough for the explanations to be interesting, so that researchers would be inclined to assent to the corresponding claims to explain.

5.2.4.2 The mechanisms of the world

We may be concerned that when an explanation uses statistical laws to explain a statistic for a large set of objects or events, the explanation may not get as close to the mechanisms of the world as an explanation that relied on non-statistical laws would do.

The concern is not a serious one in fundamental physics. Statistical laws in fundamental physics are well-embedded in tightly-knit theories, giving reassurance in the way that we noted for exceptionless laws in section 5.2.2.4. Moving up a little way to thermodynamics, the discipline of statistical mechanics serves to embed the laws very firmly in theory.

The concern is more serious in higher disciplines. It might not seem to be so at first glance. If we return to our examples, it is perfectly possible to relate not merely the formula for distributions of earthquake magnitudes but also variations in the main parameter of the formula to geological and mechanical facts, and to relate Zipf's law to facts about how to save linguistic effort.[18] That is, it is possible to propose mechanisms that may explain the statistical laws. But despite such reassurance, the concern is still a serious one. Explanations of statistical laws and of values of their parameters tend to be at least a little speculative, a fact that is itself symptomatic of some degree of distancing from the mechanisms of the world.

Despite this concern, explanations that make use of statistical laws may be the best ones that researchers can give. Most disciplines can only be conducted if researchers overlook the microphysical detail of the world. And when statistics are of interest in their own right, as they often are, the study of those statistics makes a distancing from the individual members of the relevant populations inevitable.

If explanations that use statistical laws are the best ones that can be given, researchers are likely to assent to the corresponding claims to explain. Should we then be

[18] Amitrano, "Variability in the Power-Law Distributions of Rupture Events: How and Why Does *b*-value Change?"; Ferrer i Cancho and Solé, "Least Effort and the Origins of Scaling in Human Language".

concerned that claims to explain patterns in sets of specific occurrences or in large groups of individuals may come to be accepted when no good explanations have really been given? We should not be so concerned so long as it is clearly understood, by all who make and who read the claims, that the nature of explanation is limited by the fact that statistical laws have been used. The use of statistical laws involves a retreat from the level of specific occurrences or individuals, the level at which mechanisms might operate, but so long as that retreat is noted, a claim to explain should not be open to misinterpretation as a claim to have explained by reference to specific occurrences or individuals. Moreover, the constraints on what happens in the world that explain the contents of statistical laws are also the constraints that limit what may happen in specific cases. And it is often possible to show how results at the aggregate level are derived from results in specific cases. A degree of contact with the mechanisms of the world can therefore be maintained.

Turning to claims that play explanatory roles, a claim can draw perfectly good support from its playing such a role, even when statistical laws are involved. If the claim were incorrect, its omission from the explanans would be likely to leave the explanation in at least as good a state. Then the condition set out in section 5.1.2.2, that explanations in which a claim plays an explanatory role need to be better than explanations of the same phenomenon in which it does not do so, would not be satisfied. The claim would then not gain support from its explanatory role. But if the condition were satisfied, the claim could rightly gain support in that way.

This argument depends on our being able to say that statistical laws that connect real-world phenomena cannot be manufactured at will so as to give explanatory roles to

all sorts of claims. But we can say this. Statistical laws are testable just like non-statistical laws. There may be more scope to claim that apparent exceptions are to be blamed on atypical samples, but if enough testing is done, a statistical law may come to be established or refuted beyond reasonable doubt.

This observation as to testability does not only bear on the question of support for claims that play explanatory roles. It also helps us to have confidence in claims to explain made on behalf of explanations that use statistical laws. There is scope to test those laws, and to the extent that they pass rigorous tests we may have confidence in their correctness, supporting our confidence in the worth of explanations that use them.

5.2.4.3 Contrastive explanation

Explanations that use statistical laws can be contrastive in the same way as explanations that use laws with exceptions, by showing high likelihood. This is so even when explananda set out facts that the explanantia expressly allow could have turned out differently, albeit with low probabilities of that happening. This can give reassurance in the same way as when laws with exceptions are used. If explanations show how it was very likely that the claims in their explananda should be correct rather than alternative claims, that is some indication that the explanations get to grips with the workings of the world. There are also detailed arguments for the possibility of specific types of contrastive explanation.[19]

[19] Kuorikoski, “Contrastive Statistical Explanation and Causal Heterogeneity”.

5.3 Causal explanation

Causal explanations give a strong impression of getting to grips with the workings of the world. We shall first identify the explanations that interest us here as those that are based on general causal claims. We shall then consider types of mechanism that may be identified. Then we shall turn to implications for our confidence.

A consequence of our focus on our main question is that we shall not be interested in metaphysical questions about the nature of causation. We shall take the ways in which researchers talk of causes at face value. We are concerned with whether they should say what they do say, given their systems of thought and expression (including their concepts of causation). We are not interested in whether they should think in different terms. Thus for example any interest in the use of techniques like graphical causal models will be an interest in the rigour that they may help to impose on causal thinking, rather than in the scope to use them to support any particular view of causation.[20]

It follows that we shall be as free in our use of the concept of causation as researchers in relevant disciplines tend to be. The concept is used very freely in the social sciences, and reasonably freely in biology and chemistry, but its role in physics is debated.[21]

[20] For the use of graphical causal models and other techniques to support a particular view of causation see Hausman, *Causal Asymmetries*. We discuss graphical causal models in section 8.3.5.4.

[21] See for example Frisch, "No Place For Causes? Causal Skepticism in Physics".

5.3.1 General causal claims

We are here interested in explanations in which there are general causal claims within explanantia that combine with claims of particular fact, and perhaps with additional general claims that are not themselves causal, to forge connections to explananda that are at least reasonably strong. We shall regard the general causal claims as spelt out, even if they are in practice left implicit. We shall not be interested in singular claims that some phenomena caused others where those claims are not backed up by general causal claims. Such unbacked singular claims assert links between the phenomena that they relate, rather than explaining those links. They therefore give rise to explanations that are essentially narratives. We shall however say something about the use of causal claims that are not general in section 5.4.3.5.

Our category of general causal claims cuts across the many varieties of cause that can be argued to exist and the different ways in which it can be argued that a causal claim may be established, so we shall not map out those varieties and ways.[22]

5.3.2 Mechanisms

In addition to using general causal claims, an explanation may identify mechanisms that show how the workings of the world make the causal claims correct. We shall explore the concept of a mechanism, and then turn to implications

[22] For some of the varieties and ways see Cartwright, *Hunting Causes and Using Them: Approaches in Philosophy and Economics*, chapters 1 to 4.

for our confidence of the use of general causal claims and of the identification of mechanisms.

If an explanation is to identify a mechanism, it must point to influences of the occurrence of some phenomena on the occurrence of others, and the links between phenomena must be well-attested independently of the particular explanation. Physical pressure, as when the parts of a machine push, pull and turn one another, is the obvious case, but the influence of oxidizing agents, or of evolutionary pressures such as competition for food, or of newspaper articles on political opinions, can all qualify.

We here take a generous view of the notion of a mechanism, because the identification of any mechanisms within the scope of this generous view should have at least some favourable effect on our confidence in claims to explain and claims that play explanatory roles.[23] Moreover, there is no need for a mechanism that is identified in order to explain the occurrence of phenomena to be well-attested as a complete mechanism independently of the particular explanation. It is perfectly acceptable for only the component mechanisms to be well-attested elsewhere, while their combination in an overall mechanism is unique and the course of events thereby explained differs from any other course of events.[24] But our generosity is not

[23] This generous view also helps us to set to one side delicate questions about the different effects on our confidence of the identification of mechanical models and the identification of mechanisms in the world. For the distinction, and its importance in a different context, see Matthewson and Calcott, "Mechanistic Models of Population-Level Phenomena".

[24] An example is provided by McAdam, Tarrow and Tilly, *Dynamics of Contention*. In chapter 1 the authors set out their approach of identifying recurrent component mechanisms that come together in different ways on different occasions, leading to markedly different overall courses of events.

unbounded. We should for example have doubts about the explanatory worth of the broad-brush descriptions of mechanisms that are offered by world-systems theory, at least in part because the links between phenomena are not well-attested independently of the explanations given.[25]

A description of a mechanism can identify parts that push, pull and turn other parts like the pieces of metal in a machine, or at the other extreme it can simply map causal influences in detail.[26] The mapping of causal influences can however be a substantial achievement. Not only are causal maps difficult to establish and useful as guides to further research. They can also reveal more than one might expect. For example, they can reveal the role that is played by organization.[27] More generally, they may reveal that the structure of the relevant part of the world as studied by the relevant discipline is not such that when mechanisms are identified, there is a descent to a lower ontological level that makes reference to higher levels redundant. It may be that high-level description remains essential to understanding, as for example when social structures need to be kept in view in order to understand the actions of individuals, or when social and psychological accounts need to be given in order to make full sense of what might be regarded as, at bottom, the activity of neurons.[28]

[25] See for example the mechanisms supposedly at work in recent decades that are outlined in Wallerstein, *World-Systems Analysis: An Introduction*, chapter 5.

[26] For an argument that in some contexts at least, mechanisms can be identified with causal structures see Steel, *Across the Boundaries: Extrapolation in Biology and Social Science*, chapters 2 to 4.

[27] For organization's role in explanation see Kuorikoski and Ylikoski, "How Organization Explains". The key factors they identify in section 5, concerned with the nature of components and the relations between them, are factors that would be revealed by detailed causal maps.

[28] Ylikoski, "Micro, Macro, and Mechanisms"; Thagard, "Cognitive Science" (b), pages 602-604.

There is a scale from mechanisms that are clearly machine-like to mechanisms that are far from machine-like.[29] Daniel Nicholson draws a distinction in the context of biology between machine mechanisms and causal mechanisms. Machine mechanisms set out the workings of assemblies of parts, while causal mechanisms set out the steps that lead to the occurrence of given phenomena. Nicholson goes on to argue against an ontic view of causal mechanisms, and in favour of an epistemic view.[30] Jaakko Kuorikoski draws a similar distinction in the context of social explanation.[31] The distinctions drawn by Nicholson and by Kuorikoski are not quite the same, partly because the components of a biological mechanism are likely to be molecules, while the components of a social mechanism can easily be institutions. Thus for example the analysis of the money supply mechanism that Kuorikoski presents looks like one of Nicholson's causal mechanisms rather than one of his machine mechanisms.[32] But both authors convey the point that mechanisms can be more or less machine-like. We may borrow this thought for our purposes, using Nicholson's terminology.

There is little prospect of giving general rules to determine where a given mechanism should be placed on the scale. Dropping down a level of abstraction from descriptions of the phenomena to be explained will often be a sign

[29] There are also conceptions of mechanism that encompass mechanisms right along the scale: Illari and Williamson, "What is a Mechanism? Thinking About Mechanisms *Across* the Sciences".

[30] Nicholson, "The Concept of Mechanism in Biology". The definitions are on page 153, and the arguments mentioned here are in sections 5 and 6.

[31] Kuorikoski, "Two Concepts of Mechanism: Componential Causal System and Abstract Form of Interaction", page 144.

[32] Kuorikoski, "Two Concepts of Mechanism: Componential Causal System and Abstract Form of Interaction", pages 148-149.

that a mechanism is machine-like.[33] Another sign that a mechanism is machine-like is that the claims that explain how the mechanism works are elements of the corpus of the relevant discipline that are used across a large part of the discipline. But ultimately, decisions as to the extent to which a mechanism is machine-like will come down to the judgement of researchers.

There is a parallel scale that runs in the same direction as the scale from machine mechanisms to what Nicholson calls causal mechanisms (that is, mechanisms that are not machine-like). The first part of this parallel scale, corresponding to the extreme machine mechanism end of the other scale, covers mechanisms that show basic forces of physics at work. Mechanisms of that type are confined to the bottom end of the scale of natural sciences. The next part of the scale covers mechanisms the operation of which can be seen as bearing relationships to what fundamental physics says goes on in the world, such that the relationships would if spelt out be intelligible. (The requirement is only that the relationships would be intelligible if, hypothetically, they were spelt out. There is no need for a proper reduction to physics to be practical, and it would not even matter if such a reduction turned out to be impossible in principle.) That part of the scale, where researchers can think in terms of intelligible (if unidentified)

[33] A drop in the level of abstraction may be simply a matter of components in mechanisms being entities that are less abstract than elements in the phenomena that are to be explained, but there are other possibilities too. Kuorikoski, "Two Concepts of Mechanism: Componential Causal System and Abstract Form of Interaction", pages 146-147, emphasizes the idea of looking into a mechanism, identifying component operations, and associating them with component parts. This may be seen as a form of dropping down a level because a description of a whole machine that concentrates on what it does is more abstract than a description of its parts, their functions and their arrangement.

relationships to fundamental physics, stretches up into substantial parts of biology. Beyond that part, there is a part of the scale that covers mechanisms which are such that it would not be feasible even to gesture at intelligible relationships between their operation and what fundamental physics said went on in the world. These mechanisms are likely to be causal mechanisms rather than machine mechanisms. The mechanisms identified in some parts of biology, most of psychology and practically all of the social sciences and the humanities are found in this last part of the scale.

We shall now illustrate trends in the natures of identifiable mechanisms as we go up the scale of disciplines.

In the natural sciences, it is often perfectly feasible to gesture at relationships to fundamental physics. It might be impractical to spell out those relationships, but it is for example perfectly clear that chemical reactions proceed as they do because of how particles behave, and that cells behave as they do because of chemical reactions that take place within and around them. Moreover, exploration some way down the path from chemical reactions to particles, or from cells to chemical reactions, can be a great help in the search for machine mechanisms.

As we move up the scale, it can remain possible to identify machine mechanisms. For example, a machine mechanism might be identified in evolutionary biology if changes over time in the relative frequencies of alleles in a population were attributed to a mechanism of individual organisms moving around, and in consequence mating or in consequence not mating. It is also feasible to gesture at relationships between events in the lifespan of an individual organism and what fundamental physics says goes on in the world. But larger-scale and longer-term factors that affect

the evolution of species as wholes, and that are harder either to relate at all precisely to specified machine mechanisms, or to relate to fundamental physics even to the extent of gesturing, may also be cast in the role of causes. These factors include genetic drift and migration.[34] Explanations that cite such longer-term and larger-scale factors may gradually shade into accounts that are not primarily causal, but are primarily functional or narrative. Specifically, there is lively debate as to whether there are causal processes at the level of populations.[35] But that does not rule such explanations out of court. We should also note that large-scale explanations may not take over entirely. In biology it has been argued that explanatory factors of both local and large-scale types need to be blended, rather than segregated for use in separate explanatory exercises. This blending is proposed both in order to allow better explanations to be available at any given time, and in order to allow progress to be made.[36] In neuroscience and psychology it has been argued that a hierarchy of levels of organization and a hierarchy of levels of explanation need to be distinguished, making room for several different levels of explanation of phenomena at a single level of organization.[37]

[34] Losos et al. (eds.), *The Princeton Guide to Evolution*, section 4, particularly section 4.1: Hedrick, "Genetic Drift"; section 4.3: Ronce, "Geographic Variation, Population Structure, and Migration".

[35] Millstein, "Natural Selection and Causal Productivity"; Wang, "Is Natural Selection a Population-Level Causal Process?".

[36] Laland, Odling-Smee, Hoppitt and Uller, "More on How and Why: Cause and Effect in Biology Revisited". The sort of blending that is discussed in that paper is by no means a simplistic running together of elements from two or more separate explanations. Rather, specific forms of connection between explanations at different levels are identified. For example, the genetically determined behaviour of individual members of a species may modify the environment. The modified environment may then favour genetic variations that would not otherwise have been favoured.

[37] Johnson, "The Relationship Between Psychological Capacities and Neurobiological Activities".

Moving up into the social sciences, all hope of gesturing at relationships to what fundamental physics says goes on in the world is lost. In economics for example, we find factors that are remote from any connection with what fundamental physics has to say being cast in the role of causes. For example, fluctuations in non-rational optimism and pessimism, fluctuations in the money supply, rational economic expectations, changes in technology and changes in government regulations have all been cited as causes of business cycles.[38] But it may still be possible to identify machine mechanisms. For example, economists might explain an increase in aggregate demand for investment as caused by improved business sentiment, and then identify a mechanism in which individual business owners would review their sales figures, changes in their costs and newspaper stories about the state of the economy, and would then decide whether to make new investments. It is also possible to see some of the mechanisms identified by analytical sociologists as machine mechanisms, although it is easier to see them as causal mechanisms.[39]

One more complication is that mechanisms can be described in ways that represent what goes on in the world more or less accurately. Descriptions that do not give accurate accounts can nonetheless be essential to the practice of disciplines. For example, organic chemists can describe reactions by setting out mechanisms that fairly characterize the dynamics of reactions, or they can misrepresent those dynamics by analysing reactions into stages at which

[38] Knoop, *Recessions and Depressions: Understanding Business Cycles*, chapters 4 to 7.

[39] Examples can be found in the chapters of Manzo (ed.), *Analytical Sociology: Actions and Networks*. Mechanisms are especially conspicuous in chapter 3: Wikström, "Why Crime Happens: A Situational Action Theory"; chapter 8: Grossman and Baldassarri, "The Impact of Elections on Cooperation: Evidence from a Lab-in-the-Field Experiment in Uganda".

intermediate products arise, giving a sequence of snapshots. These two possibilities have been called the thick and thin conceptions of mechanism respectively. Thick descriptions might appear to be more satisfactory, but it is often impractical to give full descriptions that would conform to the thick conception, and even if it were practical, doing so might not benefit the work of chemists.[40]

Even when the values of variables are evidently related, there may or may not be a mechanism to be found that makes a connection between the variables. Two variables can be correlated without variations in the value of either being a cause of variations in the value of the other, and a lack of causal connection may fail to be revealed even by statistical analyses that are rather more sophisticated than the computation of correlation coefficients. For example, the methods of establishing Granger causality and of directed acyclic graphs can have this shortcoming. (We shall discuss directed acyclic graphs in section 8.3.5.4.) There is indeed a notion of "predictive causality" that is clearly understood to be a merely predictive relationship, one that does not imply a causal relationship.[41] Samantha Kleinberg has developed a sophisticated method which gives a special role to intervals of time between events, and which can identify causal connections with low rates of erroneous identification or failure to identify.[42] But even this method does not wholly overcome the worry that mathematical relationships may not be adequate evidence of causal connection. Having

[40] Goodwin, "Mechanisms and Chemical Reaction".

[41] Arjas and Eerola, "On Predictive Causality in Longitudinal Studies"; Elsner, "Granger Causality and Atlantic Hurricanes", page 480; Diebold, *Elements of Forecasting*, chapter 11, section 7. There is a wide-ranging exploration of connections between on the one hand correlation and other mathematical relationships, and on the other hand causal inference, in Woodward, "Causal Models in the Social Sciences".

[42] Kleinberg, *Causality, Probability, and Time*, chapters 4, 5 and 7.

said that, it would be bizarre to think that a method with a high success rate could achieve that success rate in a world in which there were frequently no mechanisms connecting the relata which featured in the causal claims that the method encouraged.

5.3.3 Confidence

If explanations are based on general causal claims, that indicates that they rely on an understanding of how the world actually works. That may in itself increase our confidence in the associated claims to explain. Any increase in confidence will however depend on our having confidence in the causal claims.

The identification of mechanisms can increase our confidence both in claims to explain and in claims that play explanatory roles, for two reasons. The first reason is that mechanisms show how explanations get to grips with the world. The second reason is that the identification of a mechanism that explains why a causal claim should be correct lends support to the causal claim itself. It does so by integrating the causal claim with parts of the corpus that provide explanations of how the parts of the mechanism work individually and how they combine.

We do however need to take care here. The identification of mechanisms should only increase our confidence if we have reason to think that researchers have done enough work to discriminate between parts of mechanisms and other elements in the situations in which phenomena occur. They must not fabricate mechanisms by using whatever facts about situations happen to be available. We shall not pursue that line of enquiry, although we may note that the problem of how to discriminate does receive serious

technical consideration.[43]

The identification of some types of mechanism should have a greater favourable effect on our confidence than the identification of other types. The identification of machine mechanisms can do more for our confidence than the identification of causal mechanisms, because the identification of machine mechanisms is particularly strong evidence that explanations get to grips with how the world works. The identification of mechanisms which are such that it is possible at least to gesture at relationships to what fundamental physics says goes on in the world should do more for our confidence than the identification of mechanisms for which that is not possible, because such gesturing indicates that explanations are in touch with the basic physical workings of the world, even if at one or more removes. That is a strong form of getting to grips with how the world works.

5.4 Explanation using principles

In disciplines that are high up the scale, the laws that are available do not on their own suffice to make the connections that need to be made in order to give satisfactory explanations. It is therefore necessary to make extensive use of principles, that is, claims that give general guidance on what is to be expected (see section 5.2.1). In this section we shall consider the nature of explanations that use principles as general claims in their explanantia, controls over the giving of such explanations, and implications for our confidence.

[43] An example is Baetu, "Filling in the Mechanistic Details: Two-Variable Experiments as Tests for Constitutive Relevance".

5.4.1 How principles explain

Despite the insufficiency of laws to do all the work that needs to be done by general claims in disciplines that are high up the scale, explanations are still available. For example, there are many explanations in history. Laws may play essential roles, for example when physical and chemical laws determine how much could be done by the machines that had been invented at a given time, and thereby help to explain why the rate of industrial development was limited. But such laws stand in the background.

We therefore need a new notion of explanation. The required notion is this. An account can give an explanation if it renders the explanandum both unsurprising and comprehensible. This is typically achieved by showing how the explanans and the explanandum hold together, where that requires something more than mutual consistency. Principles do much of the work in showing how they hold together. The principles may or may not be made explicit. But they must be ones that are acceptable independently of their immediate use in the explanation that is being given.

We shall now give two examples to illustrate this way of working, then consider in turn the distinction between rendering an explanandum unsurprising and rendering it comprehensible, roles for laws, the question of what more than consistency might be required, and explanations of one particularly important form, the narrative form.

5.4.1.1 Illustrative examples

A historian may explain why people with political power suddenly made large concessions to others who wanted a share in that power by noting explicitly that the threat of

violence was growing, and relying implicitly on the general principle that people fear violence enough for them to avoid it when their opponents are likely to win by force of numbers.[44]

A criminologist may explain crime by reference to the strain that arises when goals cannot be achieved by accepted means.[45] In so doing, the criminologist may rely implicitly on the principle that when people's desires are frustrated, they may resort to all manner of means to achieve their goals.

In both of these examples, the principles at work are ones that are perfectly acceptable by reference to psycho-social understandings that are in common use. This close contact with everyday psycho-social understandings is not universal. Theories that are hard to connect with everyday understandings are sometimes used in the social sciences.[46] Explanations that use principles also have roles in highly technical disciplines, such as cognitive science.[47] But reasonably close contact with everyday understandings is common. The fact that it is common means that reliance on principles is often inconspicuous. The principles do not need to be made explicit. But the reliance is no less real for that.

[44] There is an example in Davidson, *Voltaire: A Life*, page 391. The example is the agreement of the ruling class in Geneva to share power, made in March 1768.

[45] Barlow and Kauzlarich, *Explaining Crime: A Primer in Criminological Theory*, pages 58-64.

[46] A review of journals like the *European Journal of Social Theory* will confirm this.

[47] Gurova, "Principles Versus Mechanisms in Cognitive Science".

5.4.1.2 Unsurprising and comprehensible

An explanation that uses principles typically takes the principles from a wide background, and thereby relates the phenomenon to be explained to that background. The background is typically, but not necessarily, an understanding of human nature. The occurrence of the phenomenon is thus rendered both unsurprising and comprehensible, even though the explanation does not show that one should expect comparable circumstances regularly to lead to the same phenomenon.

We need to distinguish an occurrence's being rendered unsurprising from its being rendered comprehensible, in order to appreciate what it is that such explanations provide.

An explanation that only uses laws renders an occurrence unsurprising by showing that it was an inevitable or probable consequence of circumstances. An explanation that uses principles renders an occurrence unsurprising in a different sense. It shows that there was some positive reason for the phenomenon to occur, although not a reason that would make the occurrence inevitable and often not one that would make it highly probable. It also shows that the circumstances were not such as to make the occurrence very unlikely.

An explanation that only uses laws renders an occurrence comprehensible in the same way that it renders it unsurprising. Researchers set out some laws, and state that this is the way the world works. If they wish to make comprehensible the fact that the laws are what they are, they must look for deeper laws. An explanation that uses principles renders an occurrence comprehensible in two stages. It implicitly or explicitly reminds readers that the essential

participants in the situation, usually human beings, had particular characteristics, whether characteristics that were common in their species or characteristics of themselves as individuals or as members of groups. It then shows that they found themselves in circumstances which made those characteristics relevant to what they did.

The two achievements of an explanation that uses principles, rendering an occurrence unsurprising and rendering it comprehensible, rely on the same method. The invocation of principles makes connections with the natures of the essential participants.[48] If the participants act in accordance with their natures the occurrence is no surprise, and it is also comprehensible. But it is still worthwhile to distinguish the two achievements from each other. Rendering an occurrence no surprise may be conceived in probabilistic terms, even when no actual probabilities are to be had. Researchers can say that an occurrence is no surprise in the sense that if there were any way to compute a probability of its occurrence, that probability would not be especially low. It is a more elaborate achievement to render an occurrence comprehensible. Doing so requires showing the specific relevance of the natures of the participants. The difference between the two achievements can be seen in the contrast between an observer's saying "Many people might have done the same" (no surprise), and her saying "That woman founded a new movement for social reform because of her strong belief that existing social arrangements were unjust, combined with the fact that she was too hungry for rapid and radical change to work comfortably within established organizations" (comprehension). The fact that rendering an occurrence comprehensible involves showing the specific

[48] Compare the discussion of the use of principles in the giving of narratives in Munz, "The Historical Narrative", pages 857-862. Munz refers to generalizations rather than principles, and sets out his understanding of their nature on page 860.

relevance of the natures of the participants means that explanations of this type are more powerful than how-possibly explanations in the natural sciences, explanations which merely show how some result could have occurred given the laws of nature and the circumstances.[49]

Sometimes, conduct does not make sense in the light of principles that are set out in psycho-social understandings. We should be concerned at this. Researchers can only expect to make progress in the disciplines that concern themselves with human conduct if people generally act in ways that can be made intelligible. The point was made by Karl Popper when he considered the role of his rationality principle, his "principle of acting adequately to the situation".[50]

Fortunately, a good deal can be done to understand the apparently irrational, whether occasional apparent lapses of rationality or apparent systematic irrationality. But special approaches are needed, and it becomes important to reflect on the effects and the limitations of those approaches.[51] It may also be necessary to make an effort to understand the reasons for the thoughts and actions of people with apparently irrational beliefs, and to understand how their beliefs could appear to be perfectly rational when seen from their point of view.[52] Even the initial diagnosis of error in the thought of the people studied can require considerable

[49] For how-possibly explanations, and ways in which they or developments of them might do more then merely show how results could have occurred, see Persson, "Three Conceptions of Explaining How Possibly – and One Reductive Account".

[50] Popper, *The Myth of the Framework: In Defence of Science and Rationality*, chapter 8, section 12. The quoted words are on page 177.

[51] For an introduction to such approaches see Lukes, "The Problem of Apparently Irrational Beliefs".

[52] Skinner, *Visions of Politics: Volume 1, Regarding Method*, pages 40-42.

care.[53] Beyond the comprehension of apparent error in thought, further effort may be needed to understand actions that appear to be irrational even in the light of the agents' beliefs (whether those beliefs strike researchers as rational or as irrational).[54]

5.4.1.3 Roles for laws

Principles of human conduct play the dominant role among general claims that are put to work in many explanations in the social sciences and history. Such principles make the explanations distinctively social or historical. Physical laws might also play essential roles in making explanations hold together. Laws might for example limit possibilities, as with laws that limited the speed of travel given what had been invented at a certain time. Alternatively, laws might relate small-scale events to one another and thereby underpin claims that events should be linked in a chain, as when the starting of a fire is seen as leading on to the destruction of wooden houses because of laws that determine the flammability of wood.[55] But the distinctive nature of the explanations would still reflect the use of principles.

If we distinguish between dominant and other roles in this way, we can easily accommodate Wilhelm Windelband's point that general laws play essential roles even in non-

[53] Lloyd, *Being, Humanity, and Understanding: Studies in Ancient and Modern Societies*, chapter 2.

[54] For the need to be sensitive to the fact that incoherence may be real and important, rather than its appearance merely reflecting a failure of researchers to understand other ways of life, see Gellner, "Concepts and Society", sections 15 to 17.

[55] This use of laws to make chains hold together is discussed in Roberts, *The Logic of Historical Explanation*, chapter 3. The need for laws is firmly asserted on page 54.

generalizable explanations of single events. Such explanations may well need to use laws, although often without the laws or their roles being made explicit, even while principles dominate. We might expect to find explanations that did not use principles in roughly the fields in which Windelband would have identified nomothetic work, and explanations that did use principles in roughly the fields in which he would have identified idiographic work, although the match would not be exact.[56]

5.4.1.4 More than consistency

The mere consistency of explanans and explanandum is not enough to give an explanation. But it is not clear how to specify what more should be required, and it is unlikely that any one specification would apply across a wide range of disciplines.

It is the judgement of researchers that is the most appropriate guide. But to the extent that we seek a standard which is independent of judgements that might be made within specific disciplines, and which could serve as a common standard across disciplines, we may take inspiration from measures of coherence that have been developed in the context of discussions of coherentism as a rival to foundationalism.

A leading example is provided by Laurence BonJour, who takes the degree of coherence of a set of propositions to

[56] Windelband, "Geschichte und Naturwissenschaft (Straßburger Rektoratsrede, 1894)", translated as Windelband, "History and Natural Science". See the whole lecture for his views on the relationship between general laws and explanations of single events. For the nomothetic and the idiographic see page 145 (page 13 of the translation).

be increased by a high number of inferential connections between them, and decreased by division of the set into subsets that are relatively isolated from one another by a lack of inferential connections between propositions in different subsets. There are also mathematical measures of coherence. The measures that have so far been formulated have been criticized, but fortunately we need not be wedded to the details of such proposals. We need only borrow their general idea and conclude that a notion of coherence that comes in degrees, rather than being merely a notion of logical consistency, need not be a vague or insubstantial notion.[57]

5.4.1.5 Narrative explanations

A narrative sets out what happened on a particular occasion and why it happened. (We shall treat interpretations of events and analyses of why things happened as parts of narratives, rather than as separate items.) There is however usually no claim either that the events are likely to be repeated of their own accord, or that anyone could arrange for a repetition. Indeed, conceptions of history have shifted in recent centuries away from those that would see history in terms of exemplary events that might teach general

[57] BonJour's proposal is set out in BonJour, *The Structure of Empirical Knowledge*, section 5.3. For the relationship between measurements of coherence and the confidence that coherence gives see Bovens and Hartmann, "Solving the Riddle of Coherence", sections 1 to 3 and 7. Douven and Meijs, "Measuring Coherence", discusses some mathematical measures and gives references to criticisms. It is however possible to take a strong position against the possibility of defining a measure of coherence, a higher value of which would reliably indicate a higher likelihood that the beliefs which cohered were correct: Olsson, "The Impossibility of Coherence".

lessons.[58] Researchers are also unable to state laws that would explain sequences of events. Instead they must rely on principles to relate events to one another, and to allow them to see sequences as coherent and individual events as unsurprising.

We shall work with a generous concept of a narrative.[59] A narrative may set out the events that it covers in or out of chronological order, and it may set out materials in ways that allow the writer to weave substantial amounts of analysis into the account, leading the reader to see patterns and make connections. A historian may for example assemble small incidents stretching over a whole country and over more than a century, incidents that exemplify village politics, the differences between free and unfree peasants, demands from on high and protests, in order to give a narrative that allows readers to see the level and the forms of protest as consequences of the whole pattern of local life and national events.[60] Indeed, many history books written in recent decades do not have overall narrative structures in the traditional sense but treat different themes in different chapters, using traditional narratives only within those chapters or parts of them.[61] To take an example from a different discipline, a geographer may assemble details of changes in several European cities

[58] Koselleck, "Historia Magistra Vitae: The Dissolution of the Topos into the Perspective of a Modernized Historical Process". For ways in which history can still contain lessons for the present and the future see Koselleck, "Representation, Event, and Structure", especially section 5.

[59] It is for example somewhat more generous than the view that Lawrence Stone took for his purposes: Stone, "The Revival of Narrative: Reflections on a New Old History", pages 3-4.

[60] Prestwich, *Plantagenet England 1225-1360*, chapter 17.

[61] On the nature of historical narrative and the significance of the move away from large-scale narrative as a structuring device see Evans, *In Defence of History*, chapter 5, section 3.

to present patterns that make sense when seen as narratives of gentrification.[62] We may even take explanations that are designated as non-narrative by their authors, for example because they explore the interplay between recurrent factors at various times rather than giving chronological accounts, but that still set out processes of change over time, and locate them in the grey area at the edge of the vague extension of our concept of a narrative.[63]

This does not mean that any account that uses principles will qualify as a narrative. An account will only qualify if it is central to the approach taken that the objects of study were located in a story that at the time unfolded chronologically, with the absolute impossibility at any time of changing the past.[64] The chronological aspect means that when a narrative concerns creatures who might have made predictions and planned accordingly, it is most unlikely that they would have had, at any given time, well-grounded certainty as to what would happen next. Their recognition of this lack of well-grounded certainty, or the at least occasional confounding of predictions that they made, may well help to shape the narrative. And the fact that principles, and not merely laws, are at work means that a narrative is unlikely to have a sense of inevitability. The narrative up to a given time is likely to be compatible with a range of alternative developments after that time. Indeed, if the course of events narrated seems to be inevitable, a reader may suspect that the narrative is not a particularly good explanation of those events. The

[62] Smith, *The New Urban Frontier: Gentrification and the Revanchist City*, chapter 8.

[63] One example of such work is D'Errico and Banks, "Identifying Mechanisms behind Middle Paleolithic and Middle Stone Age Cultural Trajectories".

[64] Compare the comments on the need to take seriously the formative role of the unidirectional arrow of time in Munz, "The Historical Narrative", pages 852-853.

impression of inevitability may have been produced by the careful selection and presentation of facts in order to give that impression, a principle of selection that would be likely systematically to distort the picture given.

Our generous concept of a narrative means we need not fear that Maurice Mandelbaum's arguments against the idea that history is all about narrative would, to the extent that they were correct, mean that our discussion of narrative left the majority of historical work untouched.[65] (In any case, historical work that did escape our generous concept would still give explanations that used principles, so it would be within the scope of our comments on such explanations in general.) More broadly, our generous concept means that we need not take any side in recent debates over the natures, functions and capacities of historical narratives.[66] On the other hand, so long as we work with our generous concept, we cannot base arguments on distinctive features of narratives in any narrower sense.

A narrative can explain the events that it covers by fitting them together in a pattern. When researchers have constructed a successful narrative, the fact that they have done so suggests that they have selected and described objects and events in an appropriate way. The relevant notion of explanation here is the modest one of making

[65] Mandelbaum, "A Note on History as Narrative". Mandelbaum makes two main points. The first point is that the historian's objective is to represent the past accurately, not to tell an interesting story, although he does not exclude the possibility that an account may turn out to be an interesting story. The second point is that a great deal of historical work involves relating events to a standing background, rather than to other events in a temporal sequence. For an argument that Mandelbaum's position needs to be refined see Ely, "Mandelbaum on Historical Narrative: A Discussion".

[66] For these debates see Roberts (ed.), *The History and Narrative Reader*.

sense of events. This is weaker than showing how events were inevitable or nearly so. But the modesty of the notion does not mean that narrative explanations achieve little. A narrative can set events, and particularly human actions and their consequences, in a context that makes them both unsurprising and comprehensible. Such a context may even be needed simply to see how given actions should be described.[67]

We mean to bring both of the two senses of the word "colligation" under this notion of explaining events by fitting them together in a pattern. The first sense is that of the explanation of events by relating them to other events. The second sense is that of the application of organizing concepts, such as that of the Reformation, the Industrial Revolution or globalization, in order to give shape to sets of events, to demarcate the members of each set from non-members, and to facilitate higher-level accounts that relate one set as a whole to another set as a whole (as when the Reformation and the Counter-Reformation are related).[68]

A narrative may have particular interest when the pattern into which events are fitted is one that has been observed elsewhere, even though there may be no prospect of formulating a general claim that would come anywhere near having the status of a law. If narrative explanations identify such recurrent patterns, that may increase our confidence both in the corresponding claims to explain and in claims that play explanatory roles. But there is reason to be cautious. If there is too large a library of recurrent

[67] MacIntyre, *After Virtue: A Study in Moral Theory*, page 208.

[68] For the two senses of colligation and the importance of distinguishing between them see Roberts, *The Logic of Historical Explanation*, pages 16-20. Roberts himself uses the word in the first sense (pages 16-17). He explores colligation and its explanatory power in chapters 2, 6 and 7. For some hazards of colligation see McCullagh, "Colligation".

patterns, it may be too easy for researchers to claim that a given narrative exemplifies some pattern or other. Then our confidence should not be increased as much as it would be if it were in general hard to find appropriate patterns.

Narratives must be faithful to the facts in order to explain in the way that is distinctive of narratives. We must therefore be wary of narratives that, while not merely fiction, are nonetheless invented, such as invented genealogies of concepts. For example, there are accounts of political obligation that start with the making of hypothetical contracts in states of nature which never existed. Such genealogies can be very helpful in deepening understanding of the concepts involved. But their invented nature means that they do not explain how the various concepts arose or were developed, except to the extent that the invented genealogies themselves played roles in the thought of people who developed the concepts.[69]

Fidelity to the facts is a rather challenging notion when a narrative is constructed by colligation in the sense of the application of organizing concepts. The application of any such concept involves a considerable degree of interpretation. It is a decidedly less neutral act than the arrangement of events in chronological order. We should also recognize that in disciplines in which evidence tends to be sparse, such as archaeology, there is scope to debate the extent to which worthwhile narratives that do more than catalogue the evidence can be regarded as describing how things actually were, and indeed to debate whether straightforwardly factual description should be the goal.[70]

[69] For some comments on the uses of fictional genealogies see Williams, *Truth and Truthfulness: An Essay in Genealogy*, chapter 2, section 4.

[70] Wylie, “Philosophy of Archaeology; Philosophy in Archaeology”, section 1.2.

We need to be aware that not all narratives should be taken at face value, and that our understanding of claims to explain may need to be modified when explanation involves either substantial reconceptualization under the influence of organizing concepts, or the use of imaginative reconstruction to compensate for the sparseness of evidence.

Finally, explanatory narratives are given in some of the natural sciences, for example in evolutionary biology. Such narratives often have an important feature that is normally absent from narratives in the humanities and the social sciences. There is a great emphasis on substantiating the narratives by reference to statistical tests, experiments, and details such as those that may be supplied by genetics.[71] This distinctive way of working means that concerns about how narratives and other explanations that use principles can be explanatory, and about how their formulation is controlled (a topic to which we are about to turn), are less pressing than they are in the humanities and the social sciences. There is indeed a view that a majority of the strands in evolutionary biology make nomological claims rather than merely historical claims, although the sense of natural law that is involved has been queried.[72]

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[71] A browse through journals such as the *Journal of Evolutionary Biology* will confirm this. For a survey of methods that are used to help reconstruct a family tree of species and to test reconstructions see Folinsbee, Evans, Fröbisch, Tsuji and Brooks, "Quantitative Approaches to Phylogenetics". A recent example of the roles of experiments and statistical tests is McCairns and Bernatchez, "Plasticity and Heritability of Morphological Variation Within and Between Parapatric Stickleback Demes". A recent example of reference to genetic detail is Vekemans, Poux, Goubet and Castric, "The Evolution of Selfing from Outcrossing Ancestors in Brassicaceae: What Have We Learned from Variation at the S-Locus?".

[72] For the view see Bock, "Multiple Explanations in Darwinian Evolutionary Theory". For the query see Pigliucci, "Historical vs. Nomological Sciences".

5.4.2 Controls

The giving of explanations that used principles could easily be less tightly controlled than the giving of explanations that only used laws. Putative laws must on the whole pass severe tests of correctness that rely on well-established methods before they can be used to give explanations. Principles are not expected to have a measurable success rate and are not easy to isolate and test one by one, so they might easily be identified and put to work too hastily. Moreover, the degree to which an explanation that uses principles is satisfactory is less easily measured than the degree to which an explanation that only uses laws is satisfactory. There is no obvious and generally applicable standard, such as that of showing how the correctness of the claims in the explanans would increase the probability of the correctness of the claim in the explanandum by a certain amount.

Despite these difficulties, the giving of explanations that use principles can be controlled. We remarked in section 5.4.1.4 that the judgement of researchers has an important role to play. We shall now consider some more specific controls.

5.4.2.1 Controls over the use of principles

One control over the use of principles is a requirement to be able to identify the principles on which explanations rely. Principles may not be made explicit when an explanation is first given, but they should be identifiable on demand.

Control by a requirement to be able to identify principles would give an opportunity to rule that explanations were not acceptable if the principles were found to have been manufactured ad hoc, simply to allow researchers to for-

mulate explanations. Principles should be well-established, either in an everyday psycho-social understanding or within the relevant discipline or other disciplines.

Everyday psycho-social understandings are fertile sources of principles. But they will not always be enough. Researchers may need to add principles that are drawn from a more technical and less intuitive psychology. Psychohistory has developed along those lines, although the use in that discipline of psychoanalytic concepts and principles would lead some to doubt any claim that the discipline was scientific.[73] To take another example, researchers may add principles that are derived from observation of how people have in the past persuaded other people to behave in certain ways. The success of some propagandists and advertisers shows that people in appropriate circumstances can be manipulated. A principle that human beings have this susceptibility may then be used when researchers explain actions for which the ground was prepared by persuading people to make up their minds in particular ways. While such explanations are not at variance with everyday psycho-social understandings, they do require thought that is more sophisticated than could be based on those understandings alone. To continue our example, the factors that affect whether propaganda works, and the explanations of results of its use, can be complex.[74]

Researchers may also need to amend some current everyday psycho-social understanding in order to arrive at a set of principles that will allow them to make sense of a period of history, or a society, other than their own. To take

[73] For some comments on the limitations of psychohistory see Roberts, *The Logic of Historical Explanation*, pages 205-213.

[74] Jowett and O'Donnell, *Propaganda and Persuasion*. Chapter 4 discusses theories of propaganda's effectiveness. Chapter 7 presents examples of its use.

an example from history, there is an argument that if historians are to understand how the Roman Empire worked despite the limits of straightforward coercive force, they must recognize the significance of the fact that Romans responded to the demands of honour, both as possessors of it and by respecting it, in ways in which we would no longer respond.[75] Turning to anthropology, there are plenty of examples of conduct in unfamiliar societies that can only make sense if a psycho-social understanding is substantially amended.[76]

Finally, the explanatory value of different principles can be debated by researchers. For example, it might at first sight seem obvious that fear of punishment would be an important reason why people would obey the law, to the extent that it would make sense to give the notion of deterrence a central role in thought about the criminal justice system. But in fact there is an alternative way of looking at compliance, based on people's perception of the legitimacy of the law and its enforcers, although there are also studies which indicate that deterrence does matter.[77] Such debates may not yield final verdicts, but they should at least maintain a reasonable level of discipline over the use of principles to give explanations.

[75] Lendon, *Empire of Honour: The Art of Government in the Roman World*.

[76] A fine example is Douglas, *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*, chapter 8. Relationships between the unamended and the amended psycho-social understandings may be subtle and complex. See for example Williams, *Shame and Necessity*, chapter 4.

[77] On the significance of perceptions of legitimacy see Tyler, *Why People Obey the Law*; Jackson et al., "Why do People Comply with the Law? Legitimacy and the Influence of Legal Institutions". For an example of a study that argues for a deterrent effect see Abrams, *Estimating the Deterrent Effect of Incarceration Using Sentencing Enhancements*.

5.4.2.2 Controls over narrative explanations

The propriety of assenting to claims to explain in relation to narratives, and the level of support that claims may derive from playing explanatory roles in narratives, largely depend on the extent to which the creation of narratives is controlled. If it is easy to create narratives that appear to explain and to get them endorsed by researchers, then it will be easy to make claims to explain. It will also be easy to find apparently explanatory roles for claims. Then claims to explain may come to be accepted when that should not happen, and other claims will not really derive much support from playing apparently explanatory roles. But if there are strict controls over the creation of narrative explanations, then claims to explain may well be ones that should come to be accepted, and the fact that a claim finds an explanatory role within a narrative may well speak in its favour.

The endorsement of narrative explanations can be controlled by debates over their quality.[78] There are also some more specific things to say. We can identify a way to support a claim that a narrative is explanatory. We can also identify tests that are based on the addition of detail, on integration across levels of generality, and on the use of methods of analysis that are drawn from fields other than the primary field of the narrative.

[78] Several examples of historians criticizing other historians' narrative explanations, and of their criticizing interpretations that are made before formulating explanations, are set out in McCullagh, *The Truth of History*, for example on pages 74-75 (liberalism in England in the first half of the nineteenth century), 114-116 (the English Civil War), and 149-150 (the interpretation of Locke). For a classic example of criticism of a whole approach, and by implication of the credentials of claims that are made by those who use it, see Skinner, "Meaning and Understanding in the History of Ideas", sections 1 to 3.

Supporting a claim that a narrative explains

One way to support a claim that a narrative really does explain phenomena is to show how principles are put to work in linking the events that are described. Principles are often not made explicit. If a narrative concerns human beings, it is assumed that the reader will understand how human beings think. But the reader should be able to stop at any point, consider why a later event makes sense in the light of earlier events, and see which principles do what work. Examples of principles that could be noted include the principle that setbacks tend to make people wary and cautious, and the principle that when trust in others is needed, trust will be extended most readily to those who are already established friends.[79] It is however important to allow for principles on which people in a modern and sophisticated culture would not act, but on which other people did act. If the reader of a narrative appreciates that other people had principles which are now alien, and such principles are put to work in the narrative, then the narrative can make perfectly good sense.[80]

There is an important condition that must be satisfied if connections between events are to be regarded as explanatory. This is that the direction of time must be respected. People who acted at a certain time may be seen as having taken account of information they had about the past and of having been influenced by predictions. But if

[79] For an example of use of the first principle see the explanation of Metternich's caution and desire to keep ways of escape from difficulties open in Jarrett, *The Congress of Vienna and its Legacy: War and Great Power Diplomacy After Napoleon*, page 78. Use of the second principle is exemplified in Opp and Gern, "Dissident Groups, Personal Networks, and Spontaneous Cooperation: The East German Revolution of 1989", pages 673-674.

[80] Williams, *Truth and Truthfulness: An Essay in Genealogy*, pages 234-235.

some unforeseen events later occurred, choices made before they occurred should not be seen as having been influenced by the fact that they were impending. Researchers, from a still later standpoint, can very easily have their view of events shaped by their awareness of subsequent events, but they should reject narratives that rely on attributing to agents a view of events that was not available to those agents. To take one example, the assassins of Julius Caesar should not be seen as having wanted to pave the way for Octavian, because they did not know what was in Caesar's testament.[81] Researchers should also be cautious about narratives which rely on a view that agents were well-informed and clear-thinking, since they may not have been so at the time.

The addition of detail

One feature of historical narratives is that there is a ready-made framework within which to organize information. This is the temporal framework, which exists in the background even when narratives do not present events in chronological order. The temporal framework makes it easy to add detail. Each detail that is an event or a feature of an event has a time at which it can be located. Each detail that is a standing condition, such as the character of an agent or a shortage of fuel, has a span of time over which it may be relevant. Moreover, there is often a large supply of detail that could be added. The addition of detail can both increase the explanatory power of a narrative and put that power to a test.

[81] Suetonius, *Divus Iulius*, 83 (Suetonius, *Lives of the Caesars, volume 1*, pages 140-143). For references to modern controversy about what was really involved in the procedure of nominating Octavian as Caesar's heir see Osgood, *Caesar's Legacy: Civil War and the Emergence of the Roman Empire*, page 31.

The addition of detail can increase explanatory power because it allows researchers to make more use of principles that are already used, and to make use of additional principles. They can make more and more connections between events, connections that are not merely ones of temporal proximity but that also conform to expectations as to how people, institutions and societies behave. The extra connections both build up the context of each event and give additional reasons why its occurrence should not be surprising.

Such an increase in explanatory power is however not guaranteed to result from the addition of detail. It is possible to add details and find that explanations of events which were already in the narrative are not improved. There is also a risk that the addition of detail may turn out to be counter-productive, because what it contributes to explanatory power is outweighed by its effect in obscuring relationships that would be seen more easily if the narrative included less detail.

The addition of detail can test the explanatory power of a narrative in the following way. A narrative that lacks detail may explain events that it covers. As detail is added, the explanation may survive. If so, that is a good sign. On the other hand, the explanation may unravel. Details may contradict the characterizations of some objects and events, or they may show that events were not related in the ways that the original explanation required.

We may therefore say that if a narrative can comfortably integrate a lot of extra detail, and in particular if its explanatory power is maintained or even increased by the addition of detail, that is an indication of its high quality. This will add at least some support to any related claim to explain.

Other levels of generality and other methods

Narratives can sometimes be tested by checking the scope to integrate them with other narratives at different levels of generality. Historians expect accounts of the actions of individuals to fit with accounts of the polities or the societies within which they acted. And it is sometimes possible to give detailed consideration to the extent to which two or more explanatory narratives at different levels work together. This can for example be done with different narratives of evolution.[82]

Narratives can also be tested by checking the extent to which they survive the application of methods of analysis that are drawn from fields other than the primary fields of the narratives. Thus historical narratives should hold up when actions and events are analysed using methods of game theory or economic analysis, so long as the information to allow those methods to be applied is available.[83] At worst, any failures to hold up should be explicable.

5.4.3 Implications for our confidence

We want to be able to tell how far we should have confidence in the claims to explain and the claims with explanatory roles that come to be accepted when explanations that use principles are endorsed by the generality of researchers.

We shall consider three concerns. Researchers might have mistaken mindsets, the existence of multiple explanations

[82] Calcott, "Why How and Why Aren't Enough: More Problems With Mayr's Proximate-Ultimate Distinction", pages 775-779.

[83] Examples of this kind of work are given in Bates, Greif, Levi, Rosenthal and Weingast, *Analytic Narratives*.

might make it hard to claim that any given explanation was particularly good, and scope to redescribe explananda might make it too easy to give explanations.

After discussing these concerns, we shall consider two features of some explanations that use principles which may increase our confidence, both in claims to explain and in claims that play explanatory roles. These two features are explanations' having some contrastive power, and their using causal claims.

5.4.3.1 Mistaken mindsets

Researchers in a given discipline might be trapped in a mindset that was badly mistaken but that led them to endorse explanations which happened to conform to their mindset. While this is a risk, there are three reasons not to be greatly concerned. The first reason is that if that were the case, it would be unlikely that disciplines would be as successful as they are. Success would be perfectly consistent with there being considerable scope to improve existing mindsets, but success would be hard to explain if a prevalent mindset was too mistaken to be improved, and was only fit to be discarded. The second reason is that it is very likely that someone would have noticed the problem in connection with some explanations, and would have pointed out that the problem extended to a wide range of explanations. The third reason is that there is not much to be done about this risk, beyond encouraging researchers to go on being critical. If anyone occupied an elevated standpoint from which all mindsets could be appraised, it would be vital to take note of what they saw. But no such standpoint is available. The risk that is identified here is an unspecified risk, akin to the risk of transformation that we noted in section 1.1.5.3.

Having given these reasons not to be greatly concerned, we should note that researchers can share a mistaken mindset. Economic thought provides examples of occasions on which progress has required a change of mindset.[84] The fact that identifiable instances are inevitably historical should not lead us to suppose that there will be no further instances. And sometimes radical changes may take place without its being obvious that they do amount to breaks with the past. Mindsets in the social sciences and the humanities are not straightforwardly contradictory in the way that some conceptions of the world in physics, such as the classical conception and the quantum mechanical conception, are straightforwardly contradictory. It may therefore not be obvious in the social sciences or the humanities when a new mindset has consigned an old one to the scrapheap.

5.4.3.2 Multiple explanations

As we noted in section 2.1.2.1, it is a feature of disciplines relatively high up the scale that there are often several accounts of the same topic. They may well be explanatory accounts, giving rise to several explanations of the same phenomenon. Such explanations need not conflict with one another. They may simply reflect different approaches. But even then, suspicions about the quality of explanations may be legitimate. Even if all of the different explanations on offer provide interesting perspectives, some of them may be too weak for their claims to explain to be correct, especially if there are many explanations. There might easily be three good explanations of a phenomenon, but is unlikely that there would be 20.

[84] Screpanti and Zamagni, *An Outline of the History of Economic Thought*. Section 3.2 on the impact of Ricardo and section 5.1 on marginalism supply examples that are particularly striking.

When there are several explanations, there may well be no claim to explain that gains the assent of the generality of researchers. Each explanation will have its supporters. For any given explanation, the strongest claim to explain that gains general assent may take the form “J has something to contribute to the explanation of K”, where different researchers would read “something” in different ways, ranging from “a little” to “a great deal”.

It may then be difficult to pose our main question in its standard form. But we can still ask whether we should be confident that researchers act appropriately when they take the various explanations seriously.

One source of reassurance is the existence of debate about the merits of different explanations. When such debates take place, it is likely that explanations that are not to be taken seriously will be identified and will drop out of consideration. Even when there are no debates back and forth, we may gain reassurance from the explicit testing of explanations. There is also the prospect of explanations being combined, or components from different explanations being used to create new and better explanations.[85] But when there are no debates and tests are not made, we may reasonably wonder whether researchers act appropriately. Explanations which would have been found wanting if challenged might have come to be endorsed by substantial numbers of researchers without much enquiry.

So far in this section, we have considered situations in which complementary explanations are on offer. But sometimes

[85] An example of quantitative testing of explanations is Bracke and Fidora, *Global Liquidity Glut or Global Savings Glut? A Structural VAR Approach*. An example of qualitative consideration of a range of explanations, together with a discussion of the scope for combining elements from explanations (on pages 144-145), is Guo, “Democratic Transition: A Critical Overview”.

there is outright conflict. We may see this in examples of historical revisionism and the debates that they spark, such as the debate over the significance of the School of Chartres in the intellectual history of twelfth-century Europe, or the debate over how to view reconstruction following the American Civil War.[86] (We are of course only interested in revisionism of the intellectually respectable sort, the sort that aims to give better accounts than those already available. Revisionism that aims to obscure or distort history, for example for political ends, is of no interest to us at all.)

Such conflicts are to be resolved or left unresolved through debates in academic books and journals. They may sometimes be resolved by the discovery of new evidence or the development of new methods for analysing evidence. Debates are governed by the norms of the relevant disciplines, but disciplines do not have algorithms that could be specified in advance, that could work out the proper results of such debates, and that would have the decisions they produced gain the assent of the generality of researchers.

If a particular conflict is unresolved, we may well find it difficult to have confidence in claims to explain, or in claims that have come to be accepted primarily because they play explanatory roles, even when the relevant explanations are endorsed by a majority of researchers, so long as the dissenters comprise a substantial minority and are not manifestly eccentric. But there is a more general concern. The lack of a fully defined method to resolve conflicts between explanations, and the fact that some conflicts go unresolved, may make us wonder about the quality of explanations in general in any discipline that is afflicted

[86] Jeauneau, *Rethinking the School of Chartres*, chapter 1; Perman, "Eric Foner's *Reconstruction*: A Finished Revolution".

with conflicts. There is however no reason to blame the use of principles to give explanations. Rather, the natures of the objects of study in disciplines that are relatively high up the scale, and the natures of the disciplines themselves, explain both the use of principles and the difficulty of resolving conflicts, or even of fully defining a method for their resolution. When researchers study entities such as human beings, they do not usually have access to laws that could play dominant roles in explanations, causal relationships that could be confirmed by experiment, or situations that differed only in certain identified respects while being identical in other respects. It is therefore hard to define methods to resolve conflicts that could actually be used, and also sometimes hard to resolve specific conflicts to the satisfaction of the generality of researchers.

5.4.3.3 Scope to redescribe explananda

We must ask whether descriptions of explananda are adequately controlled. If they are not, it would be all too easy to redescribe explananda simply in order to make it easy to give explanations. At least some of the explanations given would then have little value. If there is a serious risk here, our confidence in claims to explain and in claims that gain support from playing explanatory roles may be diminished.

Redescription can be controlled by debate over how explananda should be described. The control is not perfect, but it can be useful. It may however only act over a long period of time, leading us to worry about misdescription in explanations that have only recently come into circulation.

An illustration of how descriptions of explananda can be debated, with consequences for the explanations that are

offered, is supplied by the Industrial Revolution of the late eighteenth and early nineteenth centuries in Britain. In the 1960s and 1970s, a view of the period as one of dramatic and wide-ranging change was popular. Then in the 1980s, new analyses of data showed that economic growth was slower than that picture would have led one to expect. The picture of the period changed over subsequent years, with a corresponding reassessment of possible reasons for the Revolution. So although descriptions can affect the range of explanations that are taken to merit serious consideration, we can derive some reassurance from the fact that debate over the analysis of evidence can lead to the revision of descriptions. On the other hand, revisions can take decades to have their full effects. Most disturbingly, the views prevalent at any one time may be affected by the circumstances of the researchers' own times.[87]

5.4.3.4 Contrastive power

We shall now turn to features of explanations that use principles which can have favourable effects on our confidence. We shall start with contrastive power, and then move on to the use of causal claims.

[87] There is an overview of the historiography, published in 2010, in Griffin, *A Short History of the British Industrial Revolution*, chapter 1, pages 5-12. There is a more detailed account of the debates of recent decades, published in 1999, in Mokyr (ed.), *The British Industrial Revolution: An Economic Perspective*, chapter 1 (the introductory chapter by Joel Mokyr). For two contrasting views on whether the new thinking of the 1980s should be adopted see Temin, "Two Views of the British Industrial Revolution"; Antràs and Voth, "Factor Prices and Productivity Growth During the British Industrial Revolution". For the point that interpretations can be affected by the times of the researchers see Cannadine, "The Present and the Past in the English Industrial Revolution 1880-1980".

Explanations that use principles are less likely to be strongly contrastive than explanations that only use laws. But an explanation that uses principles may still have some contrastive power. If the explanation leads researchers to say not merely that some phenomenon makes sense, but that it makes more sense than some plausible alternative phenomena would have made, that is a contrast. And when explanations do have contrastive power, that may have a favourable effect on our confidence in claims to explain and claims that play explanatory roles, as we noted in section 5.2.2.2.

We must be cautious. Explanations that use principles are normally constructed in order to explain phenomena that have already been identified.[88] Researchers' choice of principles to use in a given explanation, and their choice of facts to mention in the explanation, will be guided by their desire to explain the relevant phenomenon. If they had had some alternative phenomenon in mind from the start, they might have chosen different principles and different facts. Then the alternative phenomenon might well have seemed to make more sense than the phenomenon that was in fact explained. This danger, of researchers thinking that they have brought some contrast into their explanations when they have not in fact done so, is the sort of danger that is to be expected when methods of explanation are not of a kind that could be used to predict phenomena that had not so far been observed. The danger therefore extends in a modest way to researchers who give explanations in which the general claims are all laws, but who must use combinations of several laws which may interact with one another or with particular facts in unpredictable ways, as we described in section 5.2.3.6.

[88] Compare the injunction to construct historical explanations by working backward from what is to be explained in Roberts, *The Logic of Historical Explanation*, page 108.

5.4.3.5 The use of causal claims

Narratives may fit events into an intelligible pattern by using causal claims to connect them. The causal claims in question are usually one-off causal claims, rather than general claims. (We shall explain below why we do not use the standard term "singular causal claim".) We need to consider both the legitimacy of such one-off causal claims, and the potential for their use to have favourable effects on our confidence.

Legitimacy

One might expect that the respectable use of causal claims to give explanations would be severely restricted outside the natural sciences, for two reasons.

The first reason is that there would be a serious shortage of causal claims of the quality that can be found in the natural sciences. There is a tremendous loss of microphysical detail when events are described at the human scale, so unforeseen exceptions to causal claims of supposedly general application would be uncomfortably common. Opportunities to make general causal claims would be further restricted by the phenomenon of path dependence, which allows the final results of processes to be influenced quite markedly by contingent events which may very well be insignificant in themselves.[89] Very similar processes can therefore have widely differing results, making it difficult to establish any general claims by grouping similar cases.

[89] Mahoney, "Path Dependence in Historical Sociology". For some case studies see Magnusson and Ottosson (eds.), *The Evolution of Path Dependence*.

The second reason why the respectable use of causal claims might be severely restricted is that in the social sciences and the humanities, it is difficult to specify conditions for the invocation of general causal claims without giving rise to suspicion that researchers could choose when to invoke them. And if the scope for choice were eliminated, scope to invoke the causal claims might be severely restricted. For example, if it were claimed that artificially low interest rates caused asset bubbles, which asset bubbles could be explained causally by citing that general claim? In which situations would interest rates be artificially low, as distinct from their being naturally low because of investors' preferences? If that decision were left up to individual economists, it would be too easy to decide whether to explain a given asset bubble in that way or in some other way. If on the other hand a general causal claim were tightened up so as to block such manoeuvres, it might become unacceptably difficult to invoke the claim in order to give worthwhile causal explanations. If for example the causal claim were that interest rates that stayed more than 1 per cent below a rate computed in some specified market-based way for more than 12 months caused asset bubbles, there would be cases in which the claim could not be invoked even though there was strong evidence that artificially low interest rates which did not satisfy the condition had caused bubbles. There would also be cases in which the tight conditions were satisfied but bubbles were not caused because other factors intervened to frustrate the effects of low interest rates, or in which bubbles arose for other reasons. This distinction between loose and tight characterizations in the causal claims themselves is a special case of a wider distinction. If the judgement of experts is allowed to influence decisions as to when to invoke general claims to give explanations, the explanations given may be suspect. But if that judgement is excluded, worthwhile explanations may be placed out of reach.

Fortunately these difficulties afflict specific causal claims, not the use of causal ways of thinking. It remains legitimate for researchers to think in terms of causal relationships, even when phenomena are characterized in the terms of the social sciences or the humanities. Four propositions come together to support this claim of legitimacy.

The first proposition is that the physical world is made up of fields, particles and forces that are subject to laws which are such as to generate causal laws at some larger scales – for example, at the scale of molecules and their chemical behaviour.[90] (The point is made in this way because to the extent that the concept of causation is used in fundamental physics, it has features in that context which make it look decidedly strange to anyone not thoroughly familiar with physics. But we should hesitate to adopt the view that the concept of causation has no place in physics, because there is still debate about that view.[91])

The second proposition is that everything physical supervenes on that base of fields, particles and forces, in the sense that there could be no physical difference without there being some difference at the level of fields, particles and forces.

The third proposition is that while there may be behaviour at the small scale that is as good as random given the ways of making measurements that are in principle available, this effective randomness has no significant effect either at

[90] The reference to all three of fields, particles and forces involves some redundancy. But the reference to all three helps clarity, and the redundancy is harmless in our context.

[91] See for example Frisch, "No Place For Causes? Causal Skepticism in Physics". Frisch's example of electromagnetic radiation is not quite at the most fundamental level of physics, and it is also set within classical physics, but on the other hand it is not at a great distance from the fundamentals.

larger scales which start well below the human scale, or at everyday temperatures.[92]

The fourth proposition is that it is legitimate to think in terms of more kinds of causal claim than the kind that might be made in physics or chemistry, and to apply different standards to the making of different kinds of causal claim.[93]

It is therefore legitimate to talk in terms of causal relationships between states of the world as characterized at the human scale. A given (if perhaps not determinately measurable) state of some part of the world, characterized in microphysical detail, could be followed by another given state, also so characterized, in accordance with general causal claims. The two states might be characterized in the high-level terms of the social sciences or the humanities, although the production of such descriptions would be based on observations in the terms of those disciplines and not on any computation that started with descriptions in the terms of physics. Researchers could then say that the first state caused the second one, even when the states were characterized in high-level terms. The fact that researchers would not claim to have established causal laws in high-level terms would mean that the claim to have identified a causal relationship between the two states as characterized in high-level terms would not be defeated by a concern that would arise out of multiple realizability. (The concern would be that different initial states in microphysical terms could

[92] For completeness, we should note that it has been argued that quantum effects can matter on large scales and at everyday temperatures. See Hameroff and Penrose, "Consciousness in the Universe: A Review of the 'Orch OR' Theory", section 3.1 for the size of microtubules and section 4.5 for temperatures. This whole line of work is however somewhat speculative.

[93] Kutach, *Causation and its Basis in Fundamental Physics*, chapter 1, on types of causal claim and standards; Steward, *A Metaphysics for Freedom*, chapter 8, primarily on types of cause.

have had the same high-level characterization, even though that would have allowed a state characterized in the same high-level terms as the identified cause not to be followed by any state that would have been characterized in the same high-level terms as the identified effect.)

There is one more condition for it to be legitimate for researchers to think in terms of causal relationships. This is that it must be legitimate to abstract events from their contexts, and to view the extracted events in certain ways and not others, in order to identify specific causes. Michael Oakeshott opposed such abstraction and thought that the only, and yet powerfully explanatory, thing to do in history was to give as complete a recital of events as could be given.[94] But the practice of researchers in the social sciences and the humanities says otherwise. Causes do get identified, and the results of such identifications are considered to be useful.

Effects on confidence

It is not enough to show that it is legitimate to talk in terms of causal connections between states of the world. It would be a further step to identify particular causes of given phenomena. In order to have a sense of the potential for the identification of specific causal connections to have favourable effects on our confidence, we need to understand the nature of the connections that may be identified in the social sciences and the humanities.

Researchers may very well claim that some features of a situation caused some features of some later situation, where all of the features concerned are characterized in the terms of their discipline. But they are unlikely to

[94] Oakeshott, *Experience and its Modes*, pages 140-143.

be able to support their identifications of causes in ways that would make those identifications safe from challenges that were perfectly reasonable. It is not merely that the human world is a complicated one. There are specific difficulties that would stand in the way of substantiating identifications of causes by reference to patterns in data, even when researchers only sought to defend the use of their identifications to make one-off causal claims rather than general causal claims.[95] It would also be difficult to identify underlying mechanisms without leaving the identifications exposed to challenges from rival identifications that were in equally good (or bad) standing. There would for example not be much hope of seeing off rivals by pointing to laws of nature. (The limits on what might be achieved that are set out in this paragraph are the reason why we do not use the standard term "singular causal claim". There is a widespread view that such claims must be backed by causal laws, or at least by counterfactuals. And while historians, for example, do sometimes experiment with counterfactuals, the results of such work would not be sufficiently immune from challenge to supply the kind of counterfactual backing that would be required to defend identifications of causes against all reasonable challenges.[96])

This does not mean that the making of one-off causal claims has nothing to contribute to our confidence in claims to explain or in claims that play explanatory roles. The identification of causes in higher disciplines is not an undisciplined activity. There are methods that can impose rigour on the identification of causal connections in the

[95] For these difficulties see Franzese, "Multicausality, Context-Conditionality, and Endogeneity".

[96] For a discussion of the status of counterfactual history see Niall Ferguson's introduction to Ferguson (ed.), *Virtual History: Alternatives and Counterfactuals*. For a view of the importance of counterfactuals that is tied closely to causal reasoning see Levy, "Counterfactuals and Case Studies".

social sciences, and the sophistication of those methods has increased over the decades.[97] We should however note that while such methods may impose discipline, they are not guaranteed to yield conclusions of types that might be desired.[98]

In the humanities, lively debates about causes reassure us that researchers take care before assenting to claims, and we may in turn have some confidence in claims that have come to be accepted. The causes of wars, from ancient wars to recent ones, are particularly hotly debated. While debate lasts, few explanations would be widely enough endorsed either to allow the corresponding claims to explain to qualify as accepted, or to lend much support to claims that played explanatory roles. But when the dust settles, the fact that there has been lively debate may give us reassurance.

It is also important not always to seek causal explanations. Explanations of the actions of individuals are an area of particular risk. It is perfectly legitimate to identify causes

[97] A recent survey of some methods that impose rigour can be found in Morgan (ed.), *Handbook of Causal Analysis for Social Research*. Chapter 13 of that book, Elwert, "Graphical Causal Models", covers a method on which we shall comment in section 8.3.5.4. For a method of identifying events and then tracing relationships between them by asking questions about causal dependence that is less sophisticated mathematically, but that is perhaps easier to use in relation to the event-rich narratives of human affairs, see Griffin, "Narrative, Event-Structure Analysis, and Causal Interpretation in Historical Sociology". There is also the method of sequence elaboration, which can be used to discipline the construction of causal chains and the ranking of causes as more or less significant: Mahoney, Kimball and Koivu, "The Logic of Historical Explanation in the Social Sciences", pages 128-141.

[98] For example, the method of sequence elaboration cannot always be used to assess the relative importance of different causes. Such difficulties are acknowledged in Mahoney, Kimball and Koivu, "The Logic of Historical Explanation in the Social Sciences", page 142.

of wars or economic fluctuations, and to explain mundane patterns of social behaviour by reference to reasons (such as widespread subscription to a religion) that may be viewed as causes even though the conditions that those reasons identify do not change over the period of interest, but merely support continuing patterns. Researchers may think in causal terms when they think about the roles of the members of specified groups of people, whether they think of the members generally or of typical but unspecified members. The identified causes may include both changes in prevalent viewpoints, such as a growth in nationalism, and standing tendencies to behave in certain ways, such as a tendency to defer to people who occupy designated positions of authority. And there is no obstacle in principle to identifying some past experiences or some general facts about human psychology as causes of such changes or tendencies, so that an explanation of a war, fluctuation or pattern of behaviour can be causal all the way down. But when researchers consider the actions of specified individuals, explanations of those actions that concentrated exclusively on external causes would detract from the picture of individuals as makers of their own decisions and creators of their own lives. Neuroscientists might be happy to abandon that picture, but researchers in the social sciences and the humanities need to retain it. If they do not retain it they will not see the people they study as people like themselves, influenced by reasons, preferences and trivia but then deciding what to do. Explanations need to cite people's reasons for acting in ways that make those reasons appear as reasons, and not solely as causes.[99]

The identifiable causes of wars, economic fluctuations and the like may well include actions of specified individuals. So

[99] This thought sits within a wider debate about action and social meaning. For an introduction that takes a particular line see Skinner, *Visions of Politics: Volume 1, Regarding Method*, chapter 7.

the need not to concentrate exclusively on external causes when explaining such actions means that it is sometimes appropriate to welcome, and not merely to tolerate, gaps in networks of causes of large-scale events. Suppose that large-scale causes of a war which first started to have their effects decades before the outbreak of war can be identified. Close analysis shows that in the few days before the outbreak of war, certain politicians took key decisions.[100] Historians may be able to identify reasons why they took those decisions, such as worries about what their political opponents in their own countries would say. But if historians wish to give explanatory roles to those decisions, it may not be appropriate for them to concentrate solely on external causes of the decisions. If they concentrate solely on external causes, the decisions will not appear as free. But if they do not concentrate solely on external causes, the decisions can be seen as free even when causes of the difficult circumstances in which the unfortunate politicians found themselves are identified. In legal work too, it is normal not to trace lines of causation through deliberate human actions. Such actions are seen as creating breaks in causal chains.[101]

[100] For an example of such a study see Clark, *The Sleepwalkers: How Europe Went to War in 1914*. Part 2 sets out developments and tensions in the decades preceding 1914 and the natures of the decision-making processes in different countries. Part 3 concentrates on the period immediately before the outbreak of war and the actions of individual politicians.

[101] Hart and Honoré, *Causation in the Law*, page 44.

5.5 Different disciplines

We shall now consider why explanations that only use laws are standard in some disciplines, while explanations that use principles are standard in others.

5.5.1 The availability of laws

As we move up the scale of disciplines, it becomes harder to identify a range of laws that is wide enough to supply the requirement for general claims in explanations. It therefore becomes harder to use laws alone. (We discussed the reasons why exceptionless laws are hard to come by in high-level disciplines in section 3.4.1. Laws with modest numbers of exceptions are likely to be in comparatively short supply for the same reasons.)

There is no single point at which there cease to be enough laws. And there are general claims that might reasonably be regarded as laws, even if they were prone to exceptions. (As we noted in section 5.2.1, we have no precise definition to tell us what should count as a law. We seek to follow reasonable practice.) In economics for example, work on how sharp changes in government spending affect the relevant economy can yield generalizations that could, if carefully formulated and applied, approach being laws.[102] And it has been argued that there are plenty of useful generalizations which have at least some of the flavour of laws in the social sciences more widely.[103]

[102] Ramey, "Identifying Government Spending Shocks: It's all in the Timing".

[103] Goertz, "Descriptive-Causal Generalizations: 'Empirical Laws' in the Social Sciences?"

An important move in the search for laws is often to look at statistics rather than individuals. For example, if researchers study all of the participants in a workforce, it may well be possible to obscure the individuals, to measure statistical variables and to formulate laws, albeit laws that are prone to exceptions.[104] There is also some prospect of finding recurring patterns in economic history through the practice of the relatively new discipline of cliometrics, which applies sophisticated techniques of economic analysis that were designed for contemporary economies to historical data. There is even some prospect of finding recurring patterns in social history and large-scale political history under the banner of the even newer discipline of cliodynamics, which seeks relationships between, for example, population growth, educational levels and levels of social stability, as well as analysing cultural evolution by using tools drawn from the study of biological evolution. It is however not clear that general claims made in cliodynamics could reasonably be regarded as laws, given that this would require not only establishing when developments of certain types were followed by phenomena of certain types, but also when such developments were not followed by phenomena of those types.[105] Finally, it is possible to explain the availability of at least some laws at the human level despite the shadow cast by fears of multiple physical realizability

[104] It is for example possible to draw conclusions about relationships between the ages of members of workforces and the productivity of those workforces: Malmberg, Lindh and Halvarsson, *Productivity Consequences of Workforce Ageing – Stagnation or a Horndal effect?*; Vandenberghe and Waltenberg, *Ageing Workforce, Productivity and Labour Costs of Belgian Firms*.

[105] There is considerable activity in both disciplines, as may be seen from the journals *Cliometrica: Journal of Historical Economics and Econometric History*; *Cliodynamics: The Journal of Quantitative History and Cultural Evolution*. For some views on the potential of cliodynamics see Turchin, "Arise 'Cliodynamics'"; Spinney, "History as Science".

of given situations as described at the human level.[106] But while examples and arguments may show that laws can be found and explain why they are available, the distribution of laws in the social sciences is patchy. There is no reason to expect to find a coherent and comprehensive body of laws. Moving up to the humanities, the patchiness of the distribution of such laws as there are becomes even more marked.

It is therefore no surprise that explanations that only use laws should have their most significant roles in disciplines low down the scale, while explanations that use principles have their most significant roles in disciplines high up the scale. But it is not merely that laws are readily available low down the scale, in quantities that allow them to cover all or substantial proportions of the fields of study of the disciplines concerned. It is also relevant that the laws that are available, in physics and chemistry in particular, are precisely stated, and that they relate quantities which are modest in number, well-defined and measurable. This means that within such a discipline it is possible to give complete explanations by using modest numbers of laws, and to do so in a wide range of situations.

As we move up the scale, even within the natural sciences, the picture changes. Consider for example epigenetics. Even when researchers confine their attention to processes at the cellular level, the environmental conditions that trigger changes can be much harder to define with great precision than, for example, chemical reaction rates.[107] Epigenetics is by no means in a poor way. A great deal of work has

[106] Papineau, "Physicalism and the Human Sciences".

[107] Examples of such conditions are listed in the column headed "Inducing conditions" in the table on pages 140-152 of Jablonka and Raz, "Transgenerational Epigenetic Inheritance: Prevalence, Mechanisms, and Implications for the Study of Heredity and Evolution". For that paper's restriction to processes at the cellular level see page 133.

been done, and the prospects for future work are bright. But we cannot expect to find laws of epigenetics that will have the same reach within that discipline as the laws that are used in physics and chemistry have in their disciplines. Systematic explanation of practically everything within the scope of a discipline like epigenetics using laws alone may still be an aspiration, but it is not obvious that it is achievable. In any of the social sciences or the humanities, such an aspiration would clearly be unachievable.

5.5.2 Laws and human decisions

The difficulty of giving explanations that only use laws in disciplines that are relatively high up the scale need not be a loss. There is sometimes reason for researchers who study individual human beings in ways that are not purely biological to prefer explanations that use principles. The use of principles can make it possible to give accounts that allow readers to use everyday psycho-social understandings, perhaps modified to allow for differences in time or culture. In particular, the use of principles can allow readers to project onto the people studied their own internal sense of the process of weighing up options, deciding and acting.

Explanations of people's decisions and actions that only used laws would not allow that kind of projection. Such explanations would treat decisions as the forced outcomes of the operation of laws, a treatment that would not be true to human experience. Take for example the statement "He felt torn between patriotism and family loyalty". The reader would herself have a sense of the demands that the concepts of patriotism and family loyalty can make on someone who has those concepts. If a discipline represented people merely as governed by laws, it would not capture that sense of the

demands that the possession of concepts can make on those who possess them, because it would fail to give an adequate representation of someone who felt the pressure of those demands and had to decide what to do, rather than simply responding to the demands in the way that a machine would respond to pressure on parts of its mechanism. The discipline would therefore not represent the people who featured in its accounts as possessing the concepts, in a form of representation that would allow application of an everyday psycho-social understanding.

We may now consider why explanations that use principles are not subject to the same handicap. The reason is straightforward. A principle does not set out what is bound to happen, or what is bound to happen in normal cases, or what is bound to happen in a given percentage of cases. A principle merely sets out how people in general, or people with certain characters, generally act in certain types of situation, and implicitly or explicitly indicates some considerations that will count as reasons within their own deliberations. The principle leads researchers and readers not to be surprised if someone, or someone of an appropriate character, does act in that way. It also makes conduct of that nature comprehensible. But there is no sense of constraint, of an agent's being directed by nature, or even of an agent's being influenced by nature in the sense of having the probability of his acting in a certain way being at a certain level or being increased by a certain amount. An explanation of conduct by reference to principles therefore allows readers to see a free agent who makes decisions, creates himself and responds to the demands of concepts that he has adopted. If someone is seen in this way, an everyday psycho-social understanding, or perhaps a modified version of one, can be applied to him.

If however researchers do not bring someone's conduct under laws, even laws with exceptions or statistical laws, but instead refer to principles that make his conduct comprehensible, how can they be said to have explained that conduct? The answer is that the principles make reference to a psycho-social understanding and bring that understanding to bear. The agent is seen as a human being, given to human ways that researchers and readers understand from the inside because they are also human beings. That is in itself a kind of explanation.

The reasons we have given here why explanations that only used laws would not suffice in disciplines that gave accounts of individual human lives are not reasons to think that the conduct of human beings could never be wholly or largely predicted on the basis of laws. They are only reasons to think that a discipline which was dominated by explanations that only used laws would not be anything like the social sciences and the humanities as they are currently practised. But our concern is with claims that come to be accepted in disciplines as they are practised.

5.5.3 Simplification as a route to laws

We have noted why, in any discipline that is relatively high up the scale, there are not enough laws to conduct the discipline as a whole, and why a preference for laws over principles might be inappropriate. But there is still a natural urge to found a discipline on laws to the extent that this would be both possible and appropriate, because explanations that only use laws give the distinct impression of being stronger than explanations that use principles.

We shall now consider a way in which laws may be given a greater role than they would otherwise have. When the

topic is some type of human conduct, researchers may manage to identify laws if they concentrate on people's desires and make some simplifying assumptions. The result may be the creation of a theoretical variant of the relevant discipline, an approach that we discussed in section 4.4.2.

The classic example is supplied by the notion of homo economicus, a rational being who acts independently of others to maximize his own utility and whose interactions with others simply reflect his personal preferences, signals from the market on prices and signals from the market on what income can be obtained from which activities. If economists make this simplifying assumption about human beings and then explore how such simplified people would act in order to maximize their utility, it becomes possible to derive laws that would work very well in a world that was populated solely by homines economici.[108] Likewise, economists and other social scientists can use game theory to make predictions about how agents will behave, assuming that they have specific strategies with regard to payoffs (such as maximin), and that they will act rationally given the limited information that the relevant games make available to them.

Social scientists are however keenly aware that it is only an unrealistic degree of simplification that allows them to move into a world in which laws provide reasonably comprehensive coverage, as opposed to the very patchy coverage that is available in the real world. Unrealistic motives must be imputed, or unrealistic abilities to work out the most effective means to ends must be assumed, or

[108] The assumption is a radical simplification, and there is considerable scope to adopt a more sophisticated picture of human beings than the simplification would allow while still engaging in contentful and disciplined economic research. See for example Stout, "Taking Conscience Seriously"; Davis, "Competing Conceptions of the Individual in Recent Economics".

both. Social scientists therefore try not to use unrealistic simplification in ways that would lead the theories to collapse as soon as that simplification was challenged. (Simplicity is however not always placed out of reach. What matters is not that simplification should not be used, but that it should not be used in ways that would lead theories to collapse when they were challenged. Sometimes, models of the world that result from the use of unrealistic assumptions can be perfectly useful. We shall return to this topic in section 5.7.3.2.)

We shall now consider the extent to which refinement of a simplified picture, acknowledging more of the complexity of real human beings, reduces the availability of laws.

5.5.3.1 The acknowledgement of complexity

We have taken the simplifying assumptions that economic theories sometimes make about human motivation and conduct to exemplify simplification in order to allow the development and the use of laws. We shall now take behavioural economics to exemplify modification in order to recognize the complexity of human beings.

Behavioural economics recognizes that people have imperfect information and limited ability to select means to ends. People may make decisions by using rules that are quick and easy to use, but that do not always recommend the best choices. They may over-react or under-react to signals from the market, creating fluctuations that are wider than would be necessary merely in order to reallocate resources in response to changed preferences and availabilities. And they may act from a range of motives and with a range of different objectives in mind. The field of behavioural economics is wide, and it does not have sharp boundaries.

It includes studies of what happens when economic agents have ways of reasoning that are not those of homo economicus, studies of how people behave in organizations, neuroeconomics, and much else. There is also lively debate as to whether such work represents a legitimate extension of the discipline of economics.[109]

One consequence of modifying the picture that would be painted by regarding each person as a perfectly rational, fully informed and efficiently calculating homo economicus is that events that would be inexplicable on that basis become perfectly explicable. The result is however not ideal. The cost of giving a better representation of the world is a retreat from laws that have, in the imaginary worlds where they apply, a high degree of reliability, and that between them give reasonably comprehensive coverage of topics. To take one recent example, a study of wage behaviour that took account of behavioural factors considered the results of many studies and weighed up a number of factors very carefully, but in the end was far from being able to give a theory of the origins of wage rigidity of a type that would allow precise predictions to be made about cases not already studied.[110]

There is sometimes a way out of the dilemma that simplified pictures of the world are too simple, while realistic pictures make it hard to arrive at laws that have a high degree of

[109] General guides include Baddeley, *Behavioural Economics and Finance*; Altman (ed.), *Handbook of Contemporary Behavioral Economics: Foundations and Developments*; Diamond and Vartiainen (eds.), *Behavioral Economics and its Applications*. For examples of debate see the papers in Caplin and Schotter (eds.), *The Foundations of Positive and Normative Economics: A Handbook*.

[110] Bewley, "Fairness, Reciprocity, and Wage Rigidity". The fact that tools of precise prediction were not made available is no criticism of Bewley's work. Rather, this is not the sort of work that is likely to yield such tools.

reliability. Researchers in the social sciences can sometimes rule complexities out of consideration quite safely, without fear that they will thereby misrepresent the world. Safety is unlikely to spring from an acknowledgement that results will only be approximately correct. It would be most unwise to hope for that, when dealing with complex systems that may approach being chaotic in the sense that is given by chaos theory. Instead, safety may spring from the nature of the researchers' interests. For example, economists may not need a full theory of the limits on people's rationality when their concern is with markets as wholes.[111] To give another example, a mathematical model of a social network that is inevitably a greatly simplified representation of the world can still be used to characterize, and to some extent explain, how such a network changes when there is disruption such as the disappearance of an important member of the network.[112] As with the focus of economists on markets as wholes, the conclusions drawn are of types that should be relatively immune to being undermined by the degree of simplification. In this case the conclusions are about stability, information and the reduction of uncertainty, topics that are well suited to be approached through the mathematical analysis of networks. But while this sort of approach may help with the reliability of laws, it is not at all clear that it would be a route to sets of laws that would give reasonably comprehensive coverage of topics.

[111] Ross, "Psychological versus Economic Models of Bounded Rationality".

[112] Barrett, Henzi and Lusseau, "Taking Sociality Seriously: The Structure of Multi-Dimensional Social Networks as a Source of Information for Individuals".

5.6 Erklären and Verstehen

The categorization of explanations as those that only use laws and those that use principles can be compared to the long-standing categorization of pieces of work as those that offer Erklären and those that offer Verstehen. We shall first say something about Erklären and Verstehen, and then make the comparison.

5.6.1 The two concepts

The traditional translations of the words "Erklären" and "Verstehen" are "explanation" and "understanding" respectively, but we shall use the German words. We shall continue to apply the English word "explanation" more broadly, to cover accounts that are explanatory in any way.

The concepts of Erklären and Verstehen do not have contents that are both precise and universally agreed. In particular, authors tend to allow the content of the concept of Verstehen to emerge from the uses they make of it.[113] We shall therefore specify contents.

A given explanatory account may offer both Erklären and Verstehen to varying degrees, or it may offer just one of the two. An explanation that offers Erklären typically tells the reader how the regularities and the mechanisms of the world give rise to some observed phenomenon. It explains by reference to those regularities and mechanisms, and the reader understands the phenomenon on that basis. By contrast, Verstehen amounts to making sense of human conduct in a way that is broadly empathetic – although

[113] A concise indication of the complexities involved is given by Scholz, "Verstehen".

as we shall note below, mere empathy is not enough. The reader sees directly that the human agents involved acted in ways that human beings do sometimes act, and that they did so by virtue of their having goals, values and an ability to make decisions. A reader achieves Verstehen on being given enough information to understand some human conduct by combining that information with a view of the agents involved as subjects, and not merely as objects. The reader sees them as possessing human points of view, just as she possesses one, and that is a crucial commonality. For our purposes, a reader will see someone as possessing a human point of view when she sees him as investing people and events with meaning and value, as motivated by specific considerations which are of the same general type as considerations that would motivate her, as making decisions in response to investments of meaning and value, and as having a sense of being a self-directed agent rather than a cog in a machine.

We are not here concerned with whether people in fact possess human points of view, but whether the reader sees them as possessing them. (Seeing people studied as possessing human points of view must of course be productive, and it will not be productive if the people studied do not in fact possess human points of view or something very similar, so their actual nature does matter. But that is a separate concern.) The need to see people in that way is the reason why the elements listed echo the normal human condition. And we specify *human* points of view because we are interested in what it would take for a human reader to achieve Verstehen, rather than in what it might take for some non-human rational being to do so.

Within the list of elements, the reference to motivating considerations being of the same general type as those that would motivate the reader might appear to be rather

restrictive. But "the same general type" designates a type that is potentially very broad. We only require that the motivating considerations be ones that the reader could see might motivate a human being. They would not need to be ones that would in fact motivate the reader or people with whom she would be in sympathy. One could argue for a stricter requirement, that the reader would need to have an appreciation of the specific culture of the people studied which was not as detached as would be the appreciation of a complete outsider who was nonetheless human, but we do not need to argue the point. Such a non-detached appreciation of a culture would automatically imply as much as we need – the requirement for the reader to see the people studied as having human points of view.

One mark of Erklären is that an explanation is given in terms which make irrelevant the human nature of those who give the explanation and those who grasp it. The achievement of Verstehen, on the other hand, relies on the human nature of the reader who achieves it. The achievement of Verstehen draws on the fact that the reader both possesses a human point of view and sees those whose conduct is described as likewise possessing human points of view. It is this perceived common ground that allows the reader to make sense of the conduct. And it is the exploitation of commonality that makes the achievement of Verstehen a natural and efficient process. Attempts by non-human readers who were not broadly similar to human beings to achieve Verstehen of human conduct would fail because what non-human readers might think was common ground would not correspond to how the people who were studied actually saw themselves.[114] The dissimilar readers

[114] For a remark on the importance of an ethnographer's being human and a view that Verstehen will then be achievable despite the ethnographer's being an outsider see Manicas, *A Realist Philosophy of Social Science: Explanation and Understanding*, pages 64-67. Non-

would then have to fall back on some more laborious way to explain human conduct.

Our way of giving the content of the concept of Verstehen picks out one thread in a complex tradition. We are interested in the type of Verstehen that depends on the correctness of the accounts that confer it, the type that is achieved when someone reads an account and is satisfied that it shows why things happened as they did. The achievement of satisfaction must not require the reader to assent to claims that would clash with the actual corpora of relevant disciplines or with the reader's everyday psycho-social understanding (perhaps modified in ways that the reader can accept). The end result may feel, to the one who achieves Verstehen, just as good as the understanding that she would achieve by being provided with an explanation that offered Erklären. It is however distinguished by its reliance on the reader's possession of a human point of view from something else that may be given the name "understanding" in English, and that may either result from Erklären or amount to Verstehen. This latter type of understanding is characterized by Lynne Rudder Baker as involving making sense of that which is understood.[115]

human beings could be close enough to human beings for them to possess points of view that were similar enough to human points of view, but we shall not spell out this possibility each time. We shall simply take such beings to meet the requirement to possess human points of view.

[115] Baker, "Third-Person Understanding", page 186. Baker uses this point in an argument for the recognition as real of things that are not to be defined in the terms of physics, and some of those things are artefacts of human institutions. But this does not detract from the fact that understanding of the nature that she identifies can often result from Erklären. In similar vein, a notion of understanding that requires grasping how everything fits together is offered in Kvanvig, *The Value of Knowledge and the Pursuit of Understanding*, chapter 8 (the requirement is stated on pages 192-193). One development of such a line of thought can be found in the view that understanding

We have drawn the distinction between Erklären and Verstehen in terms of what the reader needs, not in terms of the nature of the results. Erklären will typically confer access to propositional information which can be shared with other human beings. The achievement of Verstehen of the type that interests us will also confer access to shareable propositional information. It will leave the reader with an understanding of why things happened in the way they did. She could articulate this understanding to another human being by setting out people's circumstances, characters, desires, worries and so on, and explaining their conduct in those terms. We are not interested in Verstehen that would not enable such articulation, but would only give the reader a feeling of empathy. Purely empathetic Verstehen is not a source of claims of the types with which our main question is concerned.

5.6.2 Laws and principles

One might anticipate a rough match between explanations that only used laws and those that offered Erklären, and likewise a rough match between explanations that used principles and those that gave readers Verstehen. But that would not get to the nub of the matter. Rather, the connection between the two categorizations is this. A distinguishing feature of Verstehen in the sense we have specified is that its achievement depends on the reader's possession of a human point of view. That is not a distinguishing feature of explanations that use principles, but it is something that is required in order for many

is achieved when one grasps how a system works without making exact calculations: de Regt and Dieks, "A Contextual Approach to Scientific Understanding", sections 4.1 and 4.2. As with Baker's notion, understanding either in Kvanvig's sense or in de Regt and Dieks's sense can either result from Erklären or amount to Verstehen.

explanations that use principles to be seen as having explanatory power, while it is only rarely required in order to see explanations that only use laws as having explanatory power.

This connection between Verstehen and explanations that use principles arises as follows. Principles are very often derived from everyday psycho-social understandings. Their explanatory power is clearly seen by readers who have human lives, lives which they lead in ways that can be made intelligible by reference to such psycho-social understandings. If readers reflect on how they live, they can appreciate that a psycho-social understanding reflects what are, from a human point of view, straightforward facts about human nature. Examples are the facts that we often act in accordance with stable characteristics, that we can be influenced by trivial events long ago, that we feel the force of desires, worries and responsibilities, and that our actions result not merely from computations but from decisions which are viewed by the people who make them as free, and for which they see themselves as responsible. Readers can also appreciate that they would themselves be influenced by many of the considerations that are cited in explanations of other people's conduct. They are well aware that considerations like loyalty, ambition and security matter.

In this way, accounts that use everyday psychological concepts make perfect sense to human readers. Such readers understand the force of reasons, preferences and concerns. There might in theory be a translation of almost everything in such accounts into terms that could also be appreciated by non-human readers, but such translations would be impossibly cumbersome. It is much more straightforward to draw on the fact that human readers directly appreciate

the force of considerations of a number of types.[116] And one aspect of human points of view would be particularly hard to replace. This is the internal sense of making decisions freely and with responsibility, the sense of being more than a cog in a machine, or even a complex assembly of cogs. Even a sophisticated notion of guidance control would be inadequate to our internal sense of how we decide and act.[117]

It might be thought that analyses of human conduct in terms of the values of different courses of action in a world of human beings who all had preferences as between outcomes, and especially game-theoretical analyses, would obviate the need for possession of a human point of view. Such analyses can be very sophisticated. They can explain a great deal.[118] They can also take account of people's predispositions to follow social norms. Herbert Gintis has set out the importance of such predispositions. There would however still be something missing. A reader might learn that people acted in certain ways on account of a predisposition and their expectations as to payoffs, but that explanation would not be compelling unless the reader appreciated that predispositions and expectations as to payoffs could move people.[119]

[116] Wolfgang Spohn argues for the related view that researchers in the human sciences must engage in normative discourse in order to do their empirical work: Spohn, "Normativity is the Key to the Difference Between the Human and the Natural Sciences", section 3.

[117] For the notion of guidance control see Fischer, "Compatibilism", section 8.

[118] Examples related to cooperation can be found in Gintis, Bowles, Boyd and Fehr (eds.), *Moral Sentiments and Material Interests: The Foundations of Cooperation in Economic Life*. For examples of the use of game theory to give historical explanations see Brams, *Game Theory and the Humanities: Bridging Two Worlds*, chapter 8.

[119] For Gintis's argument see Gintis, *The Bounds of Reason: Game Theory and the Unification of the Behavioral Sciences*, chapter 8 (chapter 7 of the 2009 edition). The need for an appropriate predisposition is spelt out on pages 143 and 153 (pages 133 and 143

In contrast to principles, the explanatory power of laws can usually be appreciated without adopting a human point of view. Laws link phenomena to one another in ways that could be evident to a wide range of rational beings on the basis of experience of the external world. (Only the general idea of how linkages worked, and the contents of a few elementary laws, would in fact be evident to most of us. Special skills are commonly required in order to make the detailed content of many laws comprehensible, but those skills are not likely to be limited to human beings.)

It is however possible for laws to have explanatory power that is only visible from a human point of view. Laws can sometimes be derived from an everyday psycho-social understanding. Researchers may for example be confident that when someone has a strong sense of having incurred some moral obligation, and he has the means to discharge that obligation without serious inconvenience, he will do so even if nobody has any way to force him to do so. That could qualify as a law of human nature. Even though the proportion of occasions on which the antecedent would fail to be followed by the consequent could not be given exactly, it might be clear that it would be below some low level, such as five per cent. That statistical fact could be clear to a wide range of rational beings. But the fact that the law was underpinned by a forceful mechanism of the world would only be evident from a human point of view, the adoption of which would disclose how someone with a sense of having incurred a moral obligation would feel impelled to discharge it.

of the 2009 edition). Gintis speaks of a normative predisposition, but that perfectly apt turn of phrase should not lead us to overlook the need for the reader of an account that refers to such a predisposition to have had the right kind of life in order to grasp the nature and the force of normativity.

5.7 Special types of explanation

In this section we shall consider three special types of explanation: those that use case studies, those that make use of results, and those that use models.

5.7.1 Case studies

Case studies are used in the social sciences. They also appear in history, often under the name of microhistory, although not all microhistory should be seen as the use of case studies.[120] We shall reserve the term "case studies" for studies that analyse what happened in given situations in some detail with a view to learning lessons, whether about the cases studied, about similar situations or about ways in which it is possible for the world to work. We shall include studies where such goals are not explicit. They are for example often not explicit in historical studies. We shall not apply the term "case studies" to illustrative examples that are only mentioned in passing. We shall also confine our attention to issues that arise from the use of case studies generally. We shall not stop to consider issues that only arise in special contexts, such as the issues that surround the use of case studies in the history of science to draw conclusions in the philosophy of science.[121]

[120] For examples from a range of social sciences see Yin, *Applications of Case Study Research*. For a discussion of the use of case studies in one type of economics see Alston, "The 'Case' for Case Studies in New Institutional Economics". For examples of microhistory and a range of perspectives on microhistory see Brooks, DeCorse and Walton (eds.), *Small Worlds: Method, Meaning, and Narrative in Microhistory*.

[121] For an optimistic view of such issues, with references to earlier literature, see Chang, "Beyond Case-Studies: History as Philosophy".

Case studies allow researchers to make use of a great deal of detail about specific examples, detail that may be unavailable for other examples or that would be unmanageable if it were to be assembled for a large number of examples. We shall now consider ways in which case studies may play roles in giving explanations, and some issues that may affect our confidence.

5.7.1.1 Explaining specific cases

An individual case study may show what happened (the explanandum), and exactly how it happened (the explanans), in the case actually studied. The scope to attend to detail can make case studies particularly useful in this kind of work.[122] And a detailed account of what happened in a particular case can bring important regularities to light.[123] We may regard this type of work as a form of the use of narratives to explain.

5.7.1.2 Generalizing and theorizing

Case studies may also be used to draw conclusions about cases generally from selections of cases that are studied in detail, either by straightforward generalization, simply taking it that the cases not studied are like those that have been studied, or by using case studies to reach theoretical conclusions that can then be used to explain what happens in cases generally. Neither generalizing nor theorizing could sensibly be done on the strength of a single case study, but a

[122] Bennett and Elman, "Complex Causal Relations and Case Study Methods: The Example of Path Dependence", section 3.

[123] Sabean, *Property, Production, and Family in Neckarhausen, 1700-1870*, page 12.

reasonable number of studies taken together could provide a respectable foundation for such work.

When straightforward generalization is used, the first stage is to give an account that is selective enough in the details it includes to allow it to apply to all of the case studies, while it still includes the details that researchers consider to have explanatory significance. Then at the stage of generalization, any claim to explain that is made on behalf of that account is simply extended to cases not studied.

There is an element of theorizing even in straightforward generalization, because details in an account are selected on the basis that they are thought to have explanatory significance. That selection will be guided by views as to how some existing understanding of the world should be applied to the case studies. But it is also possible to use case studies to substantiate theoretical claims themselves. Case studies may show how processes work in specific cases. They may thereby substantiate claims about reasons for the apparent correctness of wider-ranging hypotheses, such as the hypothesis that democracies do not go to war against one another.[124] Case studies can also help researchers to develop and substantiate theories by facilitating the close and detailed engagement of theory with evidence.[125]

There are reasons to be cautious about the use of case studies to generalize or to substantiate theoretical claims. Although these are different uses, they do have such reasons in common, so we shall treat the two uses together. We shall now look at some of the reasons for caution.[126]

[124] Crasnow, "The Role of Case Study Research in Political Science: Evidence for Causal Claims", section 4.

[125] Rueschemeyer, "Can One or a Few Cases Yield Theoretical Gains?", page 318.

[126] For a fuller discussion of what case studies can and cannot do see Gerring, "The Case Study: What it is and What it Does".

Representativeness and variation

An essential part of both generalization and the substantiation of theoretical claims is to compare a range of case studies. Careful consideration of similarities and differences can give researchers confidence in the claims that they make, or it can lead them to sound cautionary notes. When the goal is generalization, the comparison may be of a merely aggregative nature. When it is the substantiation of theoretical claims, some deeper analysis of points of similarity and difference and of the reasons for them is likely to be required. But in either case researchers should be concerned that the case studies chosen be appropriately representative, even though there is rarely much hope of taking a sample that would even approach the statistician's usual standard of randomness.[127] Two possible concerns are that too few case studies may have been used, and that the selection of case studies may have been biased.

The number of case studies matters because a few normal cases will not represent the whole field. It is important to give reasonable coverage of the range of cases, including both normal cases and cases that are for various reasons abnormal. Any general claims that are made and that do not explicitly exclude abnormal cases ought to hold for at least a reasonable proportion of abnormal cases, if those general claims are not to be misleading. (They would be misleading even if abnormal cases were rare, because a failure to exclude abnormal cases would suggest that peculiarities were irrelevant to the correctness of the claims.) The difficulty of using enough case studies is

[127] For some remarks on the design of studies that involve multiple cases see Yin, *Case Study Research: Design and Methods*, pages 56-63. A comment about not being in the business of taking samples is on page 57. There are also some remarks on the selection of case studies in Fearon and Laitin, "Integrating Qualitative and Quantitative Methods", section 2.

accentuated by the fact that in the social sciences and the humanities, there are many different points of interest about each case. The ratio of the number of case studies that it is practical to consider to the number of relevant variables within each case study is often rather low. The position need not be hopeless, because work can be done to extract a wide range of testable implications of hypotheses.[128] But we still ought not to have much confidence in any claims that should only come to be accepted on the basis of a large number of case studies, unless many case studies were in fact considered.

There is also a risk that the selection of cases may be biased. Researchers may introduce bias by setting some arbitrary criterion for cases to be of interest, such as that some variable should be within a range that they regard as normal. Or their attention may gravitate to cases with outcomes that they find particularly interesting. Or, and particularly among historians, they may select the cases for which good records happen to survive, where that survival has been a matter of hazard.[129]

There are methods that can be used to reduce selection bias. It is possible deliberately to seek out particular types of case, such as typical cases, cases in which certain variables take extreme values, and cases that exhibit unusual causal pathways.[130] More generally, there are well-understood

[128] King, Keohane and Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research*, chapter 6; Levy, "Case Studies: Types, Designs, and Logics of Inference", page 3. The value of the methods proposed by King, Keohane and Verba has however been doubted: George and Bennett, *Case Studies and Theory Development in the Social Sciences*, pages 170-178.

[129] For a discussion of selection bias see George and Bennett, *Case Studies and Theory Development in the Social Sciences*, pages 22-25.

[130] Gerring, "Case Selection for Case-Study Analysis: Qualitative and Quantitative Techniques".

norms for the design of case study research, some of which bear directly on the selection of cases and some of which bear on other tasks, such as the specification of variables and the definition of data to be collected.[131] If we have confidence that such norms have been observed, that can increase our confidence in claims that have come to be accepted wholly or partly on the strength of work on case studies. We should however note that social scientists are more likely than historians both to be self-conscious observers of such norms, and to engage in the systematic comparison of case studies in the first place. A microhistorian is quite likely to select a single case, such as a single village. This is not to say that microhistorical work is always suspect, but only that this particular source of norm-based reassurance is likely to be less readily available than with social science work, and that microhistorians should recognize the limitations of their work. They do not always do so, and some of them have been criticized for making unsupported generalizations.[132]

Interpretation

Case studies must be interpreted. It is at this stage that claims will be made as a result of generalizing or theorizing. Those claims will be used to give explanations of the phenomena observed in case studies, and perhaps of phenomena in cases that have not been studied. Our primary concern is the confidence we should have in claims to explain and in claims that play explanatory roles. We shall look at general norms of analysis, traditional

[131] Yin, *Case Study Research: Design and Methods*, chapter 2; George and Bennett, *Case Studies and Theory Development in the Social Sciences*, chapter 4.

[132] Magnússon and Szijártó, *What is Microhistory? Theory and Practice*, pages 127-131.

hypothesis testing, and the formulation and testing of fuzzy-set claims.

As with the selection of case studies and the conduct of case study work, there are general norms of the analysis of results. (As with norms of selection, self-conscious observance is more common in the social sciences than in microhistory. The observance of norms of analysis is also more important in the social sciences, given the types of interpretive claim that are typically made in them.) Norms are implicit in descriptions of standard methods of analysis that are set out in textbooks on case study work, the norms being that the methods should be used and that any adjustments to them should be well-justified.[133] Some of the standard methods relate not only to work that compares several case studies, but also to work on single case studies.

If established methods are used, that should increase our confidence. Having said that, there are two reasons to limit the increase in confidence. The first reason is that some methods are not very precisely defined, so that judgement is needed to use them. The exercise of judgement could lead to the best possible results, or it could lead to results which were not as securely based as the good reputation of the methods might lead one to expect. The second reason is that not all methods are above criticism. If a significant number of researchers consider that a method suffers from inadequacies, its use should not have as positive an effect on our confidence as it might otherwise have had.

Turning to traditional hypothesis testing, our confidence in claims may be increased if they have been tested in the standard ways. Hypotheses are tested using standard

[133] Methods are described in Yin, *Case Study Research: Design and Methods*, chapter 5, and in George and Bennett, *Case Studies and Theory Development in the Social Sciences*, chapters 6 and 8 to 11.

statistical techniques, even though case study work is not necessarily in the business of sampling and is very rarely in the business of random sampling. Any increase in our confidence should however be limited by two concerns.

The first concern is precisely that collections of case studies are not random samples. Traditional methods of hypothesis testing therefore cannot be used to give the same level of reassurance as they can give when random samples are taken. They may play their part among a range of methods to give reassurance, but they may well be inadequate on their own.

The second concern is that the small samples that typically emerge from work on case studies may be too small to make traditional hypothesis testing much of a source of reassurance. There are arguments that for certain types of test, small samples can be perfectly adequate. But such comforting conclusions rely on judgements, for example about prior probabilities of hypotheses and about precisely which hypotheses should be pitched against one another.[134]

Finally, there is a comparatively new approach that recognizes the difficulty of reaching precise conclusions in the social sciences and that introduces what we may call fuzzy-set claims, after the role that is played by fuzzy set theory.

The standard context of use is qualitative comparative analysis, in which properties of different entities are noted and relationships of set inclusion are identified. For example, political scientists might start with all of the states that had become democratic in the period since about

[134] Such concerns about the role of judgement may for example be voiced in response to the argument for the adequacy of small samples in Dion, "Evidence and Inference in the Comparative Case Study", pages 132-139.

1970. They might identify the set of such states that failed as democracies, and also the set that had low levels of economic development, a lot of division of political parties into different factions, and weak executives. They might then find that the latter set was a subset of the former set.[135]

Fuzzy set theory does not limit researchers to saying that entities are either in or out of sets. Instead, it allows them to say that entities are in sets to degrees, ranging from 1 (simply in) down to 0 (simply out). Its use allows the formulation of fuzzy-set claims. An example would be a claim about countries, that those with high values of membership of the set of countries without strong unions formed a fuzzy subset of those with high values of membership of the set of countries with weak class voting.[136]

Fuzzy-set claims are not inherently suspect. There are systematic ways to calibrate scales of membership and to assign values of membership. There can also be system in the appraisal of the extent to which evidence supports or counts against claims.[137] But there are still reasons to be cautious. One reason is that there is scope both to debate methods of calibration to be used and, in some cases, to exercise judgement when calibrating.[138] Another

[135] Qualitative comparative analysis is described in Kogut, "Qualitative Comparative Analysis of Social Science Data"; Rihoux, "Case-Oriented Configurational Research: Qualitative Comparative Analysis (QCA), Fuzzy Sets, and Related Techniques". The example of democracies comes from Rihoux, page 724.

[136] The use of fuzzy set theory is explained in Ragin, *Redesigning Social Inquiry: Fuzzy Sets and Beyond*. The example of unions and voting comes from pages 39-42.

[137] Ragin, *Redesigning Social Inquiry: Fuzzy Sets and Beyond*, chapter 5, covers calibration and the assignment of values. Chapter 3 covers relationships between evidence and claims.

[138] Ragin, *Redesigning Social Inquiry: Fuzzy Sets and Beyond*, notes

reason is that all of the different measures of the extents to which fuzzy sets are included within other fuzzy sets have their deficiencies, so that drawing firm conclusions can require their supplementation with other methods such as the inspection of scatterplots.[139]

5.7.2 Explanations that use results

An explanation of a phenomenon may proceed by identifying its results, usually with an implicit or explicit premise that the results in question were to be expected or were desirable, and perhaps with a further premise that the occurrence of the phenomenon was the most likely or the most straightforward way to produce the results. Thus the nature of human lungs may be explained by the result that oxygen gets into the blood, and some person's action may be explained by reference to the fact that it got him a better job. (The example of lungs reminds us that the existence or the nature of an object is for our purposes a phenomenon to be explained, just as much as the occurrence or the nature of an event.) Explanations that make use of results in order to explain phenomena may be called functional, or they may be called teleological. We wish to treat them all together, so we shall use the relatively neutral distinguishing feature that results do explanatory work.

We may start with explanations of the actions of people and of some animals by reference to what those actions

the lack of precise agreed standards for calibration on page 86, and goes on to describe two different methods of calibration, the direct method (pages 89-94) and the indirect method (pages 94-97). The indirect method in particular relies on "qualitative assessments of set membership" (page 96).

[139] Smithson and Verkuilen, *Fuzzy Set Theory: Applications in the Social Sciences*, section 5.4.

were intended to achieve. This form of explanation is unproblematic, so long as it is reasonable to attribute to the agents both desires to achieve certain results and the ability to work out how to achieve them. Researchers must ensure that attributions of those abilities, and of the relevant desires and character traits of the agents, are plausible. And they should ideally check that the attributed desires and character traits are more plausible than others that could have been attributed, particularly others that would have made the actions difficult to explain. But it is likely to be fairly easy to gain reassurance on those points.

There is however a cautionary note to sound. Researchers should not assume that consequences of actions were intended, merely because those consequences might have been intended. Unintended consequences may be highly desirable, but they do not explain actions. And it is not always easy to draw a boundary between the intended and the unintended. Foreseen but unintended side-effects of actions might be thought to demand special treatment at this point, but in fact they can be categorized with unforeseen consequences under the general heading of unintended consequences. Foreseen but unintended side-effects do not themselves explain the actions that led to them, although their acceptability may explain why agents did not refrain from those actions.

An important subset of cases of unintended consequences comprises examples of social institutions or practices that were not created, or are not sustained, as a result of any actions intended to create or sustain them. The institutions or practices may be explained by reference to their functions, such as the avoidance of conflict or the facilitation of trade, but can explanations that rely on the identification of functions be good ones? They can, but only if they are supported by theories that explain why useful

functions should get performed without conscious plans to perform them. Selectionist theories, the essential idea of which is that what works will survive and spread, are one possible source of support.[140] Even when such support has been provided, there may be a need for more to explain the existence of a specific institution or practice rather than merely the existence of some institution or practice that performs the relevant function. Such additional explanation might take the form of an account of historical accidents, or it might involve showing how the specific institution or practice struck the best available balance between effort and satisfactory performance of the function.

Outside the sphere of the actions of intelligent agents, explanations in which results do explanatory work must be treated with great caution. Unconscious nature has no awareness of where it is going. An explanation that makes use of results is therefore very likely merely to put a gloss on reality. Any claim that results explained a phenomenon would be very dubious. It would also be difficult for a claim to acquire support by virtue of its playing an explanatory role in such an explanation.

Fortunately, researchers are cautious about explanations by reference to results outside the sphere of the actions of intelligent agents. But such explanations are not banished entirely. The most obvious area in which to look for them is evolutionary biology, but there is plenty of opposition to them there, perhaps as a reaction to the fact that the notion of species being well-adapted to their environments makes

[140] Kincaid, "Functional Explanation and Evolutionary Social Science". For relationships between such theories and theories of biological evolution see Haines, "Evolutionary Explanations". For a discussion that brings out the complexities of unintended consequences and their explanation see Aydinonat, *The Invisible Hand in Economics: How Economists Explain Unintended Social Consequences*.

it all too tempting to formulate explanations by reference to results.[141] More surprisingly, we may find reference to results in chemistry and even in physics, for example when a process is described as leading toward some low-energy state. Such descriptions can be argued to be legitimate, but they must not be read as claiming any kind of deliberate conduct in nature.[142]

5.7.3 Explanations that use models

Many explanations use models in order to make connections between the phenomena to be explained, the particular facts that might explain them and the laws, principles and background facts that are brought into use.

There are two senses of the term "model". The first sense is the everyday one of a simplified or tidied-up image of the objects, processes, or events that are modelled. The second sense is the logician's one, models of axiomatized theories. Models in the latter sense are argued to be necessary in order to characterize the results of work in the natural sciences satisfactorily, rather than their being needed merely in order to accommodate the limited capabilities of human minds.[143]

Our concern here is the impact of the use of models on our confidence, taking the term "model" in the first sense. We shall not be concerned with the logician's sense. We shall

[141] For advocacy of such explanations see Walsh, "Teleology". For criticism of the baleful influence of a focus on results see Reiss, *Not by Design: Retiring Darwin's Watchmaker*.

[142] Birch, "Robust Processes and Teleological Language".

[143] Some of the issues are set out in French, "The Structure of Theories" and in Portides, "Models". Le Bihan, "Defending the Semantic View: What It Takes" is a recent contribution to the debate.

also not be concerned with other uses of models, for example as aids to reasoning or as tools to make computations practical.[144]

There are several competing and overlapping conceptions of models in the natural sciences, in the sense of models that does concern us here.[145] We shall not try to pick out one conception as right for our purposes, but shall instead define the models that interest us as those in which some mathematical entities are used as models because their behaviour mimics the behaviour of the objects studied well enough to make the mathematical entities useful in research.[146] It may be possible to compute the behaviour of the mathematical entities analytically, or it may be

[144] Morrison and Morgan, "Models as Mediating Instruments", sets out how models can be used as instruments of investigation, helping researchers to do more than they could do if they focused solely on theories and data. For some comments on how models can make computations practical, comments that are made in the context of particle physics but that are of broader application, see Hartmann, "Effective Field Theories, Reductionism and Scientific Explanation", sections 3.1.2 and 3.2.2. For roles that models can play in the management of data and the development of theories see Laubichler and Müller, "Models in Theoretical Biology"; Leonelli, "What is a Model? Combining Theoretical and Material Models to Develop Intelligible Theories".

[145] For a survey of conceptions see da Costa and French, *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*, chapter 3.

[146] Godfrey-Smith, "The Strategy of Model-Based Science", pages 734-738, argues that models used in biology should be seen as "imagined concrete things", rather than purely as mathematical structures. But Godfrey-Smith allows them to be mathematical structures too, so agreement with his argument would not conflict with our approach. Actual concrete objects of the same general type as the objects modelled can indeed be models, as they are in synthetic biology: Mukherji and van Oudenaarden, "Synthetic Biology: Understanding Biological Design from Synthetic Circuits". But even then, one of the aspirations is mathematical characterization of the behaviour of the models.

necessary to run simulations. We shall include models that are substantially couched in mathematical terms, even if they do not quite meet the mathematician's normal standard of complete precision.[147] The boundary between what is included and what is excluded is hazy, but it should be clear enough in practice. We shall for example not consider models that are essentially analogies drawn from everyday life, as when the analogy of a multilateral contract is used to model a welfare state as a social contract to which all citizens are party.[148] Models like that may be very valuable aids to thought, but they are not good tools to substantiate claims. At best, they may substantiate claims that certain worlds are credible possibilities.

5.7.3.1 The use of models

Having constructed a model, researchers may conduct experiments within it. They may for example see how changes in the values of some variables lead to changes in the values of others, and how processes take place within the model. In this way they will explore the model on its own terms. If they translate such explorations back to the situation modelled, they may take themselves to have explained what goes on in that situation. It is this kind of

[147] We would for example include some but not all of the models that are identified as nonformal in Morton, *Methods and Models: A Guide to the Empirical Analysis of Formal Models in Political Science*, chapter 2.

[148] Paz-Fuchs, *The Social Contract Revisited: The Modern Welfare State – Overview and Critical Report*. This kind of use of the contract analogy is to be distinguished from the study of social contracts themselves by modelling members of populations as participants in games and then using game theory. Such models do fall within our scope. For such work see Binmore, *Natural Justice*, and for a more technical treatment Binmore, *Game Theory and the Social Contract. Volume 1: Playing Fair; Volume 2: Just Playing.*

explanatory work that concerns us here. We are interested in claims to explain and in support for claims that play explanatory roles, rather than in the predictive success of models.

There are plenty of mathematical models in the natural sciences. Some complex situation in the world is modelled by some mathematically tractable entities, computations are done on them and the results are compared with empirical data. If the fit is good, or if it can be made good by adjusting parameters in the model, the model may be taken to provide an explanation of some phenomena.[149] The sense of explanation may however sometimes be a modest one that only involves showing how patterns in different phenomena are related to one another, and does not involve engagement with underlying mechanisms.

Turning to the social sciences, models are especially conspicuous in economics, and as in the natural sciences they play roles in developing theories as well as in giving explanations of phenomena.[150] But models also feature in

[149] An example of this kind of modelling from condensed matter physics is the Gaussian chain model of polymers: Kawakatsu, *Statistical Physics of Polymers: An Introduction*, chapter 2. An example from chemistry is the modelling of complex chemical systems: Royal Swedish Academy of Sciences, *Development of Multiscale Models for Complex Chemical Systems: Scientific Background on the Nobel Prize in Chemistry 2013*. An example from biology is the use of a model based on game theory to explain the coexistence of different strains of yeast and the effects of changing the concentration of glucose: Gore, Youk and van Oudenaarden, "Snowdrift Game Dynamics and Facultative Cheating in Yeast". There are several more examples from biology in Laubichler and Müller (eds.), *Modeling Biology: Structures, Behaviors, Evolution*.

[150] The range and sophistication of models in use is illustrated by Florens, Marimoutou and Péguin-Feissolle, *Econometric Modeling and Inference*. For a discussion of the use of models in economics see Sugden, "Credible Worlds, Capacities and Mechanisms". For an example of the use of models and of how models may be refined

many research papers elsewhere in the social sciences.[151]

5.7.3.2 Distance from the world

For the purposes of our main question, an important feature of models is that many of them are more or less distant from the world that they represent. They tend to be simpler than the world that is modelled, while still reflecting relevant features of the world.[152] We shall start by considering models in the natural sciences, and then move on to the social sciences.

Distance from the world gives rise to a risk of misrepresentation, but that risk is not limited to the use of models. Even in work that does not use models but deals directly with the world, the world must be characterized in some way.[153] It is then appropriate to ask whether the characterization represents the world well enough in

in order to address conflicts between models and data see Zodrow, "Capital Mobility and Capital Tax Competition".

Models are also used for forecasting, with varying degrees of success. Forecasting raises special issues, which are for example covered in Clements and Hendry (eds.), *The Oxford Handbook of Economic Forecasting*. We shall not pursue those issues, because our concern here is understanding the world rather than making predictions. Forecasting is not irrelevant to our concerns, because the consistent predictive success of a model could increase our confidence in claims about the world that were based on its use, and conversely a lack of predictive success would reduce that confidence. We can however note that fact without exploring issues that are specific to forecasting.

[151] The journal *Mathematical Social Sciences* is a good source of examples.

[152] Godfrey-Smith, "The Strategy of Model-Based Science", page 726.

[153] Weisberg, "Who is a Modeler?", section 4, distinguishes between modelling and what Weisberg calls abstract direct representation, with the latter lacking an intermediary between the representation and the world.

relevant respects. But the question arises in a particularly obvious way when models are used. Models are not to be read as straightforward descriptions of the world. This is not a matter of the results the models produce being more or less quantitatively accurate. Indeed, a loss of quantitative accuracy may be a cost of making models explanatory.[154] Rather, it is a question of the extent to which models reconceptualize the world in ways that are not tightly constrained by a requirement to say how the world really is. And the assumptions that are made in order to construct a model may create a gap between the model and any accepted theoretical description of the world there may be. A model may even be at variance with the best theory that is accepted at the time when the model is constructed.[155]

It is the fact that models can be far from accurate descriptions that raises issues for us. We may have confidence in claims to explain that are based on the use of such models, but only so long as the claims recognize the limitations of work with models. It is important not to assume without further argument that successful models show how the world really works. It is possible for a model to predict outcomes correctly even if it does not correctly identify the stages that the mechanisms of the world follow. And if researchers do assume that models show how the world really works, they may reach ontological conclusions that result from assumptions made when creating the models.[156]

[154] Bokulich, "Explanatory Models Versus Predictive Models: Reduced Complexity Modeling in Geomorphology", section 5.
[155] Hartmann, "Effective Field Theories, Reductionism and Scientific Explanation", section 3.2.2.
[156] Winther, "Fisherian and Wrightian Perspectives in Evolutionary Genetics and Model-Mediated Imposition of Theoretical Assumptions".

A consequence of the potential for failure to describe the mechanisms of the world is that we may have only limited confidence in claims that play explanatory roles, unless it is recognized that the claims are only to be read as claims within models rather than as claims about the world.

Having raised this concern about claims within models, we should recognize that it can sometimes be overcome. It may be overcome for any one of three reasons. The first reason is that sometimes the distance from straightforward description that researchers can be confident does not misrepresent the world is short enough that there is little cause for concern. The second reason is that a model may set out a mechanism, and researchers may have reason to think that what is set out does closely represent the relevant mechanism in the world. The third reason is that when several models are integrated with one another, or when models are integrated with experimental work or with mechanistic explanations, this may indicate that the models in question should be seen as describing the world accurately, or at least as coming close to doing so.[157] Accurate description of the world would be a likely reason why descriptions of different types or from different points of view should fit together.

We shall now turn to the question of distance from the world in the social sciences. The fact that the phenomena studied can often be described in everyday terms makes it easy to suppose that models explain the world directly, but caution is required. We should take seriously the scope to reconceptualize which is suggested by Milton Friedman's argument that economists should neither want nor expect explanatory models to represent the world accurately, even

[157] Green, "When One Model is Not Enough: Combining Epistemic Tools in Systems Biology"; Brigandt, "Systems Biology and the Integration of Mechanistic Explanation and Mathematical Explanation".

if we do not agree with his argument.[158]

Concerns about the risks of reading claims to explain and claims that play explanatory roles as relating to the world rather than to models should therefore be just as strong in the social sciences as in the natural sciences. The widespread and successful use of models to advance disciplines does not show that the models in question give accurate pictures of the world. They may present credible worlds, they may be analogies, or they may be open formulae that are at some remove from making definite claims about the world.[159]

Even when such doubts are assuaged, we should not assume that models give anything approaching full representations of the world. Even in the natural sciences, models can give representations of the structure of the world that are only partial.[160] We may expect representations to be even more markedly partial in the social sciences. Even those who adopt a pretty positive view of models in the social sciences still acknowledge that models involve a distancing from the world, for example by idealizing in order to isolate factors that are of particular interest.[161]

We only urge caution, not rejection. It is perfectly possible to make a case that a model shows how the world works. One way to make such a case would be to show that the model could be integrated with other models or with

[158] Friedman, "The Methodology of Positive Economics".

[159] Sugden, "Credible Worlds, Capacities and Mechanisms"; Gilboa, Postlewaite, Samuelson and Schmeidler, "Economic Models as Analogies"; Alexandrova and Northcott, "Progress in Economics: Lessons from the Spectrum Auctions", section 4.

[160] Da Costa and French, *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*, pages 48-52.

[161] Mäki, "MISSing the World. Models as Isolations and Credible Surrogate Systems".

survey or experimental work. And even if a model does misrepresent the world, it is not clear that the model is thereby debarred from being explanatory.[162] There are ways to be explanatory that do not require accurate description of the mechanisms at work in the world.[163] To the extent that the issue is felt to be a pressing one, it is sometimes possible to close the gap between models and the world by using narratives.[164] And some claims that are made on the strength of work using models, for example claims about how the values of variables are related to one another in general and not merely in observed cases, may be claims to which it is perfectly proper to assent even if the models do not reflect how the world works. One possible source of reassurance would be to show that claims held up under variations of the model's potentially unrealistic assumptions.[165]

Finally, we can gain reassurance about claims that are based on modelling from the fact that there is plenty of scope for the systematic validation of models and for their critical appraisal.[166]

[162] For some arguments on this point see Reiss, *Philosophy of Economics: A Contemporary Introduction*, chapter 7, pages 127-141.

[163] Sugden, "How Fictional Accounts Can Explain".

[164] Morgan,"Models, Stories and the Economic World", section 5.

[165] Kuorikoski, Lehtinen and Marchionni, "Economic Modelling as Robustness Analysis".

[166] On validation in biology see Haefner, *Modeling Biological Systems: Principles and Applications*, chapter 8. Some examples of criteria that can be used in appraisal in the context of the social sciences are given in de Marchi, *Computational and Mathematical Modeling in the Social Sciences*, pages xx-xxi. There is an extended discussion of the appraisal of models in political science in Morton, *Methods and Models: A Guide to the Empirical Analysis of Formal Models in Political Science*, chapters 4 to 8 and section 9.1. For an example of testing a model see Clark, "Residential Preferences and Neighborhood Racial Segregation: A Test of the Schelling Segregation Model".

5.7.3.3 Simulations

Simulations are sometimes used to obtain results from models, either because equations are too intractable to solve analytically or because simulation is a way to make a model as faithful as possible to the world. Simulations play important roles in both the natural and the social sciences. To take just a few examples, chemists simulate ensembles of molecules in order to work out their behaviour, meteorologists simulate the atmosphere, oceans and land masses in order to predict the weather, and social scientists simulate groups of people in order to work out how they might behave.[167] This last example is one in which simulation is a way to make a model as faithful as possible to the world. The world to be modelled is a population of autonomous agents who will behave as they individually see fit, so the best approach is to construct a model that comprises simulated agents and then run simulations in which they decide individually what to do.

The use of simulations raises epistemological issues that have been debated extensively.[168] We shall not enter into wide epistemological debate here. We shall however note that the use of simulations, like the use of models generally, involves work with something that is not the world about which claims are made. The crucial issue for our main question is therefore the same as with models generally. When claims to explain and claims that play explanatory roles are made following work with simulations, how

[167] Cramer, *Essentials of Computational Chemistry: Theories and Models*, chapter 3; Randall et al.,"Climate Models and Their Evaluation"; Gilbert, *Agent-Based Models*; Helbing (ed.), *Social Self-Organization: Agent-Based Simulations and Experiments to Study Emergent Social Behavior*.

[168] For an introduction with references see Parker, "Computer Simulation".

boldly or modestly should those claims be read? Are the simulations close enough to the world in relevant respects to allow the making of claims to explain what goes on in the world, or to credit claims with roles in explaining what goes on in the world?

The concern comes into sharp focus when we take note of the view that simulations can themselves be arguments that certain results will be observed in the world.[169] A simulation might be an argument for a conclusion to the effect that given a situation of the type for which the simulation was designed, and given the specific facts of that situation (corresponding to initial values in the simulation), certain results would transpire. But whether the simulation would provide evidence of how the world would achieve those results would depend on whether the simulation's algorithm appropriately represented the workings of the world. (This question of how results would be achieved is separate from the question of whether they would be achieved. Even if a simulation made correct predictions every time, its algorithm might still misrepresent the workings of the world.) The sophistication of work on simulation methods, and the care that is taken to consider the appropriateness of models, give some reassurance.[170] But researchers' primary concern may be that simulations should yield correct predictions. Any concern that the algorithms used should correctly represent the workings of the world may then be an instrumental one: accurate representation may give the best prospects for obtaining correct predictions.

[169] Beisbart, "How Can Computer Simulations Produce New Knowledge?"

[170] The sophistication of work and the care taken can be seen by reading papers in relevant journals, such as the *Journal of Computational Chemistry* and the *Journal of Artificial Societies and Social Simulation*.

We should also recognize a special feature of a particular type of simulation, the type in which individual steps cannot be seen as rational. This type of simulation has been discussed by Tyler Burge.[171] What follows is mainly a summary of his discussion. We start with simulations in which individual steps can be seen as rational, and then consider simulations in which this is not so.

Repeated simulations of non-rational behaviour, such as simulations of the behaviour of simple animals, are simulations in which each step taken by the researcher is rational. The individual runs of the simulation constitute acts of data generation, just like acts of data collection when measurements are taken from the world. The rationale for those acts is perfectly well understood. The rationale for the computations that are made using the data is also perfectly clear.

There are other simulations in which such a grasp of the rationality of the stages is not available. These are simulations in which natural processes of evolution are simulated in order to discover the best solutions to problems. The picture is one of the survival of the fittest solution, with solutions evolving both by taking on features of other solutions in the way that organisms inherit characteristics of their parents, and by mutating at random. While there is rational justification for setting up such a process, the specific steps that take place cannot be understood as rational ones. Individual random acts of breeding and mutation are not in themselves rational. The steps are therefore not ones that the researchers can justify individually by reference to their understanding of what detailed steps it would be rational to take in order to obtain results. They can only justify the conduct of a sequence of steps of that nature and

[171] Burge, "Epistemic Warrant: Humans and Computers", pages 502-506.

reliance on the final result, on the basis that the method of simulation has been found to work well. This may limit the extent to which researchers can explain why they should conclude that the solution which emerges triumphant is optimal. They may still have ample grounds for believing that it is optimal, but their grounds are more restricted than the grounds that are available when they can explain the rationality of all of the steps that led to the result.[172]

We are unable to reach a general conclusion as to our proper level of confidence when simulations have been used. We can however use our catalogue of concerns to help us ask some appropriate questions in specific cases.

Finally, we may note one point that need not detain us. There is a mathematical question as to the extent to which results obtained from simulations, or from the use of any numerical methods, may not be what would have been obtained by analytic methods. All that is necessary for our confidence not to be reduced by that concern is for us to have confidence that researchers will have made allowance for such effects when formulating their claims.

[172] Burge distinguishes between justification and entitlement. He claims that in such a situation researchers may only have entitlement to their conclusion (“Epistemic Warrant: Humans and Computers”, pages 505-506), because justification would require having a rational argument without the gaps that the non-rational steps would create. But they would still be justified in affirming their conclusion, even if they would only have entitlement to the conclusion itself.

Chapter 6

Norms and Concepts

6.1 Norms

We introduced the norms of disciplines in section 1.3, and said something about their nature and about types of norm in section 2.1.1.2. Now we shall discuss their role in giving us confidence.

Norms govern both the conduct of research and the appraisal of claims that are candidates for assent.[1] A single norm can have influence at both stages, because a fundamental norm of appraisal is that appraisers should consider whether research was conducted in accordance with appropriate norms. They will therefore refer to specific norms of the conduct of research. Conversely, researchers will want their claims to have a good prospect of coming to be accepted, so they will make sure that they observe

[1] The scope for norms to play these two roles is evident from a review of the journal *Accountability in Research: Policies and Quality Assurance*. Papers in it cover both the conduct of research and the appraisal of claims.

the norms of conduct. A widely held desire to have the results of work receive the assent of others can indeed lead to the coordination of standards of work.[2] Moreover, specific norms of appraisal, such as the norm that claims should be checked for consistency with the corpus, can also be observed by those who intend to make claims.

Given the scope for norms to have influence at both stages, we shall not distinguish between the uses of norms at the two stages except when there is special reason to do so. The influence of norms at either stage can have a favourable effect on our confidence. Some incorrect claims will slip through processes of appraisal, so it helps that the observance of norms at the stage of conducting research will reduce the number of incorrect claims that are presented for appraisal.

We shall first review the position in disciplines low down the scale, where deductive relationships between propositions and relationships that are almost as strong are plentiful. We shall then move on to disciplines higher up the scale, where strong relationships are not so plentiful.

6.1.1 Disciplines low down the scale

6.1.1.1 Mathematics

Mathematics must as usual be treated differently from other disciplines. It does not purport to be about the physical world. It is a free-standing system of claims. We can set out the main norms as follows.

[2] Albert, "Methodology and Scientific Competition".

- Claims may be made if they have been deduced from claims that are already in the corpus.

- In addition, new types of mathematical object may be introduced, so long as their properties are defined precisely and so long as their introduction does not allow contradictions to be deduced. Deductions that involve those new types of object may then be made.

- Claims may not be made in other ways, except as conjectures that are used to guide research. Such conjectures may in due course come to command wide assent, perhaps because of their fertility, their good fit with some areas of the corpus, and the failure to find counter-examples despite an extensive search. But they cannot join the corpus until they have proofs.

These norms should ensure adequate control over whether researchers assent to claims.

6.1.1.2 Other disciplines

In other disciplines that are rich in strong relationships between propositions, the norms are more varied than in mathematics. They may range from very general norms such as “Researchers must record ways in which errors might arise”, to very specific ones such as “Researchers who use instruments to take measurements, such as colorimeters, or who use a scale with which data are to be compared, such as a scale that will determine radiocarbon dates, must calibrate their instruments or scales”.[3]

[3] Skoog, Holler and Crouch, *Principles of Instrumental Analysis*, chapter 1, section 1D; Pollard, “Measuring the Passage of Time: Achievements and Challenges in Archaeological Dating”, pages 152-154.

There may also be norms that the presence or absence of specific signs should be considered before making claims. For example, if a causal claim looks worth considering, it may be important to consider the consistency of the connection between the supposed cause and the supposed effect under different conditions, and the plausibility of possible mechanisms through which the connection could operate.[4]

Positive support for claims

The norms of any discipline will only allow researchers to make or assent to a claim if it has adequate positive support and there are no strong reasons to doubt it. What types and levels of support for claims are adequate will depend on the norms of the discipline, but if deductive relationships between propositions and relationships that are almost as strong are plentiful, those norms are likely to require support to be set out in arguments that primarily rely on such strong relationships. (The support may come from new evidence. There is no suggestion that new claims must be deduced or come close to being deduced from the existing corpus alone.)

References to positive support might sound dangerous in the light of Karl Popper's argument that theories can only be falsified, not verified, so that the most positive thing

[4] The examples are numbers 2 and 6 from the list of nine given by Bradford Hill: Hill, "The Environment and Disease: Association or Causation?", pages 296-297 and 298. Hill was however careful to regard them as viewpoints from which questions of causation could be considered, rather than as strict rules (page 299). Indeed, he expressly noted that failure to identify a plausible mechanism should not disqualify a causal claim (page 298). For a discussion of Hill's proposals in the context of more recent thought see Joffe, "Causality and Evidence Discovery in Epidemiology".

that may be said about them is that they are corroborated by their having withstood attempts to show that they are wrong.[5] This is a convenient point at which to explore the justification for thinking in terms of positive support, and indeed support that can be enough to make it legitimate simply to conclude that certain claims are correct.

We can render the notion of positive support legitimate in the context of the natural sciences by taking into account the context within which pieces of work are done. There is usually a vast background that both supplies theoretical resources and allows the immediate problem to be defined. For example, if the question is that of how disordered segments affect the lifespans of proteins, there is a vast biochemical background that sets out the roles of proteins, how they are structured, why their lifespans are important, how disorder is to be quantified, what mechanisms of influence are plausible, and how instruments and procedures can be used to obtain information.[6] A consequence is that evidence can show that some claims should straightforwardly be regarded as correct. The background makes two different contributions to making this possible. The first contribution is that it allows the establishment of probative links from evidence to claims. The second contribution is that it narrows down the options for claims that are worthy of consideration. It may do so sufficiently to answer the multiplicity-of-causes objection to the use of eliminative-causal reasoning, the objection that while all options but one of those considered may be eliminated, other options may not have been considered.[7] There is a remote risk that

[5] Popper, *The Logic of Scientific Discovery*, chapter 10.

[6] The example comes from van der Lee et al., "Intrinsically Disordered Segments Affect Protein Half-Life in the Cell and during Evolution".

[7] Achinstein, *Evidence, Explanation, and Realism: Essays in the Philosophy of Science*, chapter 11, section 3, subsection B.

the background corpus is incorrect in some relevant way, but that is not enough to make it illegitimate for researchers to say that some claims have been shown to be correct. Nor is it enough to reduce our confidence in accepted claims.

Some claims may be established on the basis of crucial experiments that decide between rival claims. Here an additional concern arises. Can claims be tested in isolation? Pierre Duhem argued that it was not really possible to test an isolated hypothesis. The need to make and interpret observations and to work out their implications means that tests rely on taking a whole theoretical background for granted.[8] More recently, Willard Van Orman Quine used his image of a fabric of belief to claim that no statement was an island. When there is tension between different statements, for example, when a statement of an observation conflicts with a statement of the relevant prediction of a theory, there is at least some scope to choose what adjustments to make in order to resolve the tension. (Quine wrote of contact between experiences and statements, rather than between statements of experience and other statements, but in the current context nothing turns on that distinction.) If the theory is far away from the edge of the fabric, and much else in other parts of the fabric depends on it, it is unlikely that one would discard the theory. One might then consider adjustments to statements in parts of the fabric that lay between the theory and the observation, including statements that embodied information on how to interpret observations. This flexibility is however not only a way to preserve theories. For Quine, no statement in the fabric was safe from challenge. Even laws of logic might be changed if that were the best way to resolve enormous tensions.[9]

[8] Duhem, *La Théorie physique. Son objet et sa structure*, part 2, chapter 6, sections 1 and 2.

[9] For Quine's whole approach see Quine, "Two Dogmas of Empiricism", section 6. Quine went much further than Duhem. For an

Such concerns make it hard to see any experiment as crucial in the strict sense of being absolutely decisive. An experiment might appear to favour one claim and refute another, but there would seem to be scope to make wider adjustments in order to avoid refuting the second claim. Data would apparently not dictate what to do: theories would seem to be underdetermined by data.

One response to this concern would be to draw attention to an implicit norm in the natural sciences that choices which would require widespread changes to the corpus should not be made without very good reason. The standard set by this norm is not often met. A variant on that response would be to borrow Hasok Chang's point that researchers get considerable guidance on how to advance their disciplines from the systems of practice of their disciplines.[10] A second response would be to dispute the legitimate extent of the concerns that would make it hard to see any experiments as crucial. This can be done. For example, Jody Azzouni has argued that sciences enjoy a degree of autonomy from one another because they need to have their specific methods, methods that are not to be derived from theories which might be woven together to form the fabric as a whole.[11] A consequence of this autonomy is that not all of the fabric is exposed to change at once. A third response would be to say that the interplay between general claims and experimental evidence was more complicated than portrayed by the

outline of a range of positions that can easily but dangerously be run together under the single banner of holism see Moulines, "The Ways of Holism", particularly sections 1 and 2.

[10] Chang, *Is Water H_2O? Evidence, Realism and Pluralism*, section 4.1.1. Chang's view on this point is connected to his views on truth, in particular his use of truth$_5$, described in section 4.3.1, and to his views on knowledge and realism, but his point can be borrowed whether or not one follows him on those matters.

[11] Azzouni, *Knowledge and Reference in Empirical Science*, part 1, section 3.

traditional picture of deducing a consequence of a claim and then performing an experiment. The traditional role for crucial experiments might not need to be performed. Such a line of argument might for example be based on the error-statistical approach that Deborah Mayo has put forward, although exactly how much work that approach could do is debated.[12]

The first two responses would only make it conceivable that experiments should be crucial in the strict sense of being absolutely decisive. Neither of those two responses would exclude the possibility of the results of experiments still leaving researchers with choices to make. If that happened, researchers might turn to norms that theories with certain characteristics should be preferred, but they might not get enough help from norms to decide between rival theories. Milena Ivanova argues that we cannot expect consideration of the virtues of theories, virtues such as simplicity, explanatory power and fertility, to allow researchers to make conclusive choices between theories, partly because researchers do not have settled ways to measure the extents to which theories exhibit such virtues, and partly because there is no settled ranking of virtues by their importance.[13]

[12] See the papers in Mayo and Spanos (eds.), *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*, and particularly chapter 1: Mayo, "Learning from Error, Severe Testing, and the Growth of Theoretical Knowledge"; chapter 2: Chalmers and Mayo, "The Life of Theory in the New Experimentalism"; chapter 4: Worrall and Mayo, "Theory Confirmation and Novel Evidence".

[13] Ivanova, "Is There a Place for Epistemic Virtues in Theory Choice?", section 3. Ivanova goes on to use rival theories of quantum mechanics as an example, in sections 3.1 and 3.2. For an argument that we cannot expect the problem of choice between theories to be solved by a requirement for researchers to exhibit the bon sens (good sense) that Duhem identified (*La Théorie physique. Son objet et sa structure*, part 2, chapter 6, section 10), either in the form of the quality that he identified or in any one of a range of forms offered

There is the further question of whether the possibility that choices may be left open should adversely affect our confidence. The extent of underdetermination of theory by data is a contested question, and we shall not settle it here. We shall merely note that if there were specific areas in which the risk were serious, that would reduce our confidence, and that there are arguments that underdetermination is not particularly widespread.[14]

by other authors, see Ivanova and Paternotte, "Theory Choice, Good Sense and Social Consensus". The authors go on to offer their own interpretation of good sense as "choosing a theory so as to smooth the scientific consensus building process" (page 1124), and they argue that this interpretation might allow the exemplification of good sense to help research communities to come to agree which theories to choose (pages 1125-1127). But as the authors freely admit, several difficulties would remain (pages 1127-1130). Moreover, an interpretation of good sense as the promotion of consensus might mean that the application of good sense could only be seen as a way to eliminate disputes, rather than as a way to make correct choices between theories.

For some more optimistic views on virtues and underdetermination see Axtell, "Bridging a Fault Line: On Underdetermination and the *Ampliative Adequacy* of Competing Theories"; Tulodziecki, "Epistemic Virtues and the Success of Science", sections 4 and 5.

Finally, there are arguments that researchers will in fact tend to favour theories that explain a wide range of phenomena, that explain phenomena in detail and that are elegant: Diamond, "Science as a Rational Enterprise". Whether such preferences are conducive to making correct choices between theories is a separate question. There are results in formal epistemology that encourage a preference for simple theories, as we shall see in section 8.3.5.4. But those results show that such a preference will promote efficiency. They do not show that simple theories are correct.

[14] Two fairly recent discussions of the question of underdetermination are Bonk, *Underdetermination: An Essay on Evidence and the Limits of Natural Knowledge*, which concentrates on analysis of the notion of underdetermination and on connections with other philosophical debates, and Norton, "Must Evidence Underdetermine Theory?", which sets out reasons to think that the arguments for general underdetermination, as distinct from arguments for its occasional appearance, are too weak to establish their conclusions.

Despite the risk that evidence, the corpus and norms may not always suffice to reach conclusions, those resources do allow researchers to make use of evidence, and particularly experimental evidence, in ways that allow claims to be given very strong support. But it is only possible to obtain strong support if evidence is obtained and used appropriately. As well as general norms, the natural sciences abound in specific rules that dictate how experiments and other exercises in gathering data should be designed and conducted, and how data should be analysed. Instruments must be calibrated, supplies of chemicals must be checked for purity, possible extraneous influences must be excluded, control groups must be used, and analyses of data must use appropriate statistical tests. What matters for our confidence is that the norms should be both clear and strict.

The intent of general norms like "Record ways in which errors might arise" is clear, but they are made strict in their application by being filled out with specific norms like "Compute and report the probabilities of different types of error, given the size of the sample you have taken". By and large, the norms of design, conduct and analysis are strict, even if they are not all codified for each discipline but are largely embodied in the understanding of experienced researchers and evidenced by the descriptions of work done that are included in published papers. It also helps that many methods are highly technical, making it obvious they will not give useful results unless they are used with great attention to detail. And it is reassuring that when experimental protocols are debated, those debates are conducted in such technical terms that only experts could hold their own.[15] Finally, the tests of statistical significance that must be used are well-established.

[15] For an example of such a debate see Mann, "Analyses of Protein S Function" and the response, Hackeng, Seré and Rosing, "Misconceptions About Protein-S Multimers".

A further reason for optimism is that many norms have been used over long periods of time, and in different contexts. If they were ineffective, researchers would have noticed. And norms can themselves evolve in response to an appreciation of their worth, as for example did norms of how to conduct trials of medical treatments once randomized controlled trials were introduced and then became commonplace.[16] It is however easier to see specific norms as open to evolution than it is to see very general norms as open to evolution, at least if we hope to see a course of evolution that we can be confident will represent rational progress. One way to see general norms as open to evolution would be to link changes in methods to changes in the aims of scientific work. But the idea of changes in aims is controversial, to say the least.[17]

Finally, the fact that methods and reasonably specific norms may well evolve should not undermine our confidence, at least not in any way that could be made precise enough to allow detailed investigation. The history of science strongly suggests that some of the methods and

[16] Stolberg, Norman and Trop, "Randomized Controlled Trials", page 1539. A further stage of evolution, still in progress, is to develop the fresh thinking that may be needed about the use of randomized controlled trials to establish what will work in the population at large. Cartwright and Munro, in their paper "The Limitations of Randomized Controlled Trials in Predicting Effectiveness", go so far as to propose a move away from the traditional notion of external validity. But it may not be necessary to go so far. It is perfectly possible to identify specific reasons why there might be difficulties: Dekkers, von Elm, Algra, Romijn and Vandenbroucke, "How to Assess the External Validity of Therapeutic Trials: A Conceptual Approach"; Rothwell, "Commentary: External Validity of Results of Randomized Trials: Disentangling a Complex Concept". The identification of specific reasons why there might be difficulties should help researchers to work out appropriate responses.

[17] The connection with changes in aims is made in Laudan, *Beyond Positivism and Relativism: Theory, Method, and Evidence*, chapters 7 to 9. Laudan discusses the controversy in chapter 9.

norms currently in use will in due course be found wanting. But there is no way to tell which methods and norms will be affected, or how serious the impact will be. We have here a general worry comparable to concerns about the risk of transformation of disciplines that we set to one side in section 1.1.5.3.

6.1.2 Disciplines higher up the scale

When we turn to disciplines in which deductive relationships and relationships that are almost as strong are not plentiful, we again find that assent to claims is only expected if the claims have adequate positive support and there are no strong reasons to doubt them. What support is adequate will depend on the norms of the discipline. But those norms will need to differ from the norms that are appropriate when deductive and near-deductive relationships are plentiful. If researchers insisted that support had to be set out in arguments that primarily relied on relationships between propositions which were deductive or almost as strong, their disciplines would be severely impoverished. They must therefore use alternative norms that are still strict enough to impose adequate control over assent to claims. We shall now consider some norms.

6.1.2.1 Evidence and argument

Norms of evidence and argument are largely given in the form of descriptions of methods that can be used to get as much as possible out of evidence, while still limiting oneself to claims that are well-supported. We gave some examples

of texts that describe methods and warn against pitfalls in section 2.1.1.2.

Norms of evidence cover how evidence should be gathered in order to ensure that what is gathered is authentic, that evidence is not corrupted in the process of gathering it, and that evidence which might undermine claims is likely to be found. They may also cover how evidence should be analysed. Norms of argument cover such matters as what forms of argument are acceptable, how much evidence is needed to establish given types of claim, what makes a case strong enough to discard existing accepted claims, and how objections to a claim may legitimately be refuted. Examples of norms of evidence and argument, given by describing methods to use, can easily be found in the social sciences.[18] Norms given in the same way are also common in historical studies.[19]

We must however acknowledge that as we go up the scale of disciplines, we find fewer agreed prescriptions that dictate which methods are acceptable and which are not. In particular, there are fewer norms that govern arguments for general claims, even if there remain plenty of norms that govern the analysis of evidence. We can find proposals for norms at an intermediate level, for example norms that govern the making of claims that some writers influenced

[18] For examples in sociology see Elliott, *Using Narrative in Social Research: Qualitative and Quantitative Approaches*. For examples of norms in the study of international relations that are particularly concerned with ways to minimize bias in the selection of sources and ways to compensate for biases that may have affected the contents of secondary sources see Thies, "A Pragmatic Guide to Qualitative Historical Analysis in the Study of International Relations".

[19] For examples in archaeology see Roskams, *Excavation*; Odell, *Lithic Analysis*; Martin, Harrod and Pérez, *Bioarchaeology: An Integrated Approach to Working With Human Remains*. For examples in history see Howell and Prevenier, *From Reliable Sources: An Introduction to Historical Methods*.

others, but such proposed norms may be neither universally acknowledged nor very widely observed.[20]

The mere lack of a complete set of agreed norms for one stage in the process of making and assenting to claims need not in itself be disturbing. We may be more concerned when there is open dispute over norms. There is for example a debate in social science about the relative merits of different ways of working. On one side there is a classic text that advocates extending the rigorous forms of reasoning that are customarily associated with quantitative work to all kinds of work.[21] On the other side, there are those who do not disparage those forms of reasoning, but who argue that they cannot always achieve as much as they promise and that room must be made for other ways of working.[22] When we observe debates of this nature, our confidence in claims that have come to be accepted under sets of norms which are not the strictest among the sets in contention must be reduced at least a little, because it is clear that some researchers, supporters of the strictest set, have found fault with sets of norms that are less strict.

6.1.2.2 Psycho-social understandings

There is a norm, not often made explicit, that researchers should not endorse an account of the thoughts and conduct of individual human beings unless it comports with an appropriate psycho-social understanding (which may not be

[20] For the example of norms to govern claims of influence see Skinner, *Visions of Politics: Volume 1, Regarding Method*, pages 75-76.

[21] King, Keohane and Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research*.

[22] Brady and Collier (eds.), *Rethinking Social Inquiry: Diverse Tools, Shared Standards*.

an understanding currently in everyday use, and which may include an allowance for occasional inexplicable conduct). An account that covers the deliberate conduct of human beings will only be satisfactory if it does so comport. If it does not, there will be no justification for seeing conduct as connected with circumstances. It is a psycho-social understanding that supplies the human world's equivalent of a mechanism. Geoffrey Elton made a similar norm explicit when he required historians to stand back from their completed accounts and ask whether those accounts were plausible, although his norm was not limited to occasions on which individuals would have thought about what to do and decided on actions.[23]

If mere compatibility with a psycho-social understanding that was acceptable to readers was all that was demanded, the norm would not be much of a constraint. Authors who wanted to be taken seriously would not be likely to write accounts that exhibited incompatibility, for example by having people react adversely to good news. But there is a stronger sense in which an account may comport with a psycho-social understanding. An account comports in this sense when application of the psycho-social understanding to the initial facts makes the subsequent conduct that the account narrates seem not merely possible, but a natural result of the initial position. (The adjective "natural" is meant to capture the idea that the conduct flows smoothly, without any feeling that more explanation is needed. It is not meant to suggest that the conduct should appear to have been inevitable.) It is not compulsory for accounts to comport with psycho-social understandings in this way, but it is a good sign when they do.

[23] Elton, *The Practice of History*, page 78 (page 86 of the first edition).

It can take some effort to show how an account does comport with an appropriate psycho-social understanding in this strong sense. Take for example Callum Brown's explanation of the negative and positive reactions to Margaret Knight's 1955 radio broadcasts favourable to scientific humanism, an explanation that proceeds by supplying the context of a resurgence of Christian culture.[24] The account succeeds because the reader can appreciate how both the wartime engendering of a pro-Christian climate and the cultural puritanism that went with the austerity of the later 1940s would make a resurgence of Christian culture natural, giving encouragement to those who opposed the broadcasts to express their opposition, and how the existence of a specifically cultural revival, rather than an upsurge in belief, would leave space for those who supported the broadcasts to voice their support – an opportunity they would be likely to take, given the propensity of people to say what they think when there are no obstacles to doing so. Elements drawn from a psycho-social understanding would include the propositions that a war can incline people to seek solace in religion, that enforced austerity can engender a belief that austerity and puritanism are admirable, and that people can adopt a culture that is derived from a system of beliefs without necessarily regarding the beliefs as true. In practice such considerations are taken as read, and it would be painful to spell them out on each occasion. But even if unexpressed, they would still play their roles.

6.1.2.3 The norm of avoiding tension

One norm that is worth singling out is the norm of avoiding tension. If a new claim is presented for consideration,

[24] Brown, "'The Unholy Mrs Knight' and the BBC: Secular Humanism and the Threat to the 'Christian Nation', c.1945-60".

researchers should consider whether it is in harmony with claims that are already in the corpus. At the very least, a new claim should not directly conflict with any claims in the corpus, and it should ideally fit in well. Researchers may refuse to assent to a new claim simply because it would not be in harmony with the corpus, so that assenting to it would create tension. But they may sometimes decide that they really ought to assent to the new claim or something very like it. They may then go back and forth between the new claim and the contents of the corpus, making adjustments to both, until there is little or no remaining tension either within the old corpus or between its members and the final version of the new claim.

Adjustments to the corpus are especially likely to be a realistic option in disciplines that are high up the scale, for three reasons. The first reason is that claims in the corpus which interpret evidence rather than merely reporting it are unlikely to have the very strong support that comes from being connected to other well-established claims by relationships that are deductive or almost as strong. The evidential threshold for making a change to a claim that is at some distance from claims that merely report evidence is therefore relatively low. (This does not mean that it is low in absolute terms. It would still be a radical step to discard well-established claims, so new claims that would require such a step are likely to be scrutinized carefully.) The second reason is that the relative paucity of very strong relationships between propositions means that it is often possible to change some claims without having to make large-scale changes elsewhere in the corpus. The cost of making a change is then relatively low. The third reason is that new claims often fit more or less well with the existing corpus, rather than simply fitting perfectly or being inconsistent with it. There is therefore scope to exercise judgement as to what adjustments to make and

how extensive the adjustments should be, both adjustments to the existing corpus and adjustments to new claims. It may well be possible to limit adjustments so that they are palatable.

Reflective equilibrium

The process of making adjustments in order to reduce or eliminate tension is an example of the process of reaching reflective equilibrium. This process can be made the centrepiece of a whole epistemology, as Catherine Elgin has done.[25] We shall not go that far, but we shall find it worthwhile to explore the notion a little.

The connection between reflective equilibrium and the reduction or elimination of tension is clearest if we use Willard Van Orman Quine's image of a fabric of belief, the image that we mentioned in section 6.1.1.2.[26] Tensions may arise in the fabric when new evidence conflicts with claims that are already incorporated in the fabric, or when consequences of existing claims are worked out and are found to conflict with one another. Then researchers need to work out what adjustments to make, and they may have a choice. Such a choice will be guided by considerations like the norms that long-established claims should not be overthrown on a whim and that disruption should be limited by avoiding changes that would have far-reaching implications, but such guidance may not suffice to eliminate choice. There may be times when researchers should discard long-established claims or tolerate widespread disruption. Reaching reflective equilibrium may require striking a balance between competing considerations, and the result may not be entirely satisfactory. We should also note that

[25] Elgin, *Considered Judgment*, especially chapter 4.
[26] Quine, "Two Dogmas of Empiricism", section 6.

choice is most likely to arise in disciplines in which deductive and near-deductive relationships between propositions are not plentiful, because a shortage of very strong relationships makes it harder than it would otherwise be for existing corpora to constrain options tightly or to allow experiments or other procedures to be crucial. When it is a question of whether evidence and other considerations are on the whole strong enough to lead researchers to a particular conclusion, judgement must be exercised and choice therefore arises.

There is a considerable body of formal epistemological work on how sets of beliefs should be revised in the face of new evidence, some of it specifically on how new claims should be considered in the light of existing information.[27] But while our confidence may well be increased by awareness that researchers observe the guidance that such work gives, the guidance is at too high a level of generality, and is too dependent for its implementation on the possession of systematic lists of beliefs, for it to tell researchers precisely what to do.

6.2 Formative concepts

6.2.1 The notion of a formative concept

Formative concepts are the concepts that have a significant effect on how researchers approach the objects of study in a discipline. They may be, but need not be, basic in the sense of standing at the bottom of hierarchies of concepts

[27] Fuhrmann, "Theories of Belief Change"; Bovens and Hartmann, "Belief Expansion, Contextual Fit, and the Reliability of Information Sources".

that are built up by successive stages of definition of the corresponding terms.

Some concepts are clearly formative, for example the concept of energy in physics, the concept of evolution in biology, the concept of utility in economics, the concept of toleration in political studies and the concept of industrialization in the history of certain times and places. On the other hand, a great many concepts pick out detailed features of objects of study and are unlikely to be formative. Examples include concepts of different arrangements of leaves on plant stems (such as alternate and whorled) in biology, the concept of settlement risk as between counterparties in economics, the concept of tactical voting in political studies and the concept of sumptuary regulation in the history of legal systems. There are also concepts that may or may not be accorded formative roles, depending on the preferences of researchers. We shall consider some examples shortly.

Whether a concept is formative depends partly on the discipline. A concept may fail to be formative in a broad discipline, but may still be formative in a narrow discipline. Concepts of different arrangements of leaves might be formative in the study of the morphology of plants, the concept of settlement risk might be formative in the study of risk in financial markets, the concept of tactical voting might be formative in the study of voting systems, and the concept of sumptuary regulation might be formative in the study of class distinctions.

Our reference to the use of different concepts as formative does not mean we contemplate different choices that are incommensurable in the strong sense of leaving researchers unable to debate their choices. There will be agreement on the use of some formative concepts and on many

detailed features of the objects of study. There will also be substantial agreement on the background to the relevant discipline. This background may be supplied by a larger discipline that encompasses the relevant discipline, by adjacent disciplines, or by a shared general understanding of the world. We do not need to argue against Donald Davidson's opposition to the idea of radically different conceptual schemes.[28]

6.2.2 Choice

We shall now give some examples of concepts that researchers may choose to allow to be formative. Not every researcher in the relevant discipline need allow these concepts to shape her approach to the objects of study. Researchers who allow some concepts to be formative may argue that their preferred concepts must shape every researcher's approach in order to make progress. But others will work productively without letting the concepts in question shape their approaches. This is enough to show that allowing the concepts to be formative is indeed optional.

The examples we give are drawn from disciplines that range from biology upward on the scale. This should not surprise us. All sorts of choices are more limited low down the scale than high up the scale. We should also not suppose that a given opportunity to choose will remain open indefinitely. It may be that after some years of successful experiment with new formative concepts, the new concepts will displace older ones.

[28] Davidson, "On the Very Idea of a Conceptual Scheme". Davidson notes the need for agreement on pages 196-197.

Our first example of choice comes from the study of organisms. This study may be shaped by the concept of the forms of parts, by the concept of the functions of parts or by the concept of the development of organisms, leading to different types of explanation of the characteristics of organisms.[29] Another biological example is the option to give the concept of information a central role.[30]

Evolutionary biologists may shape their approach by using the concept of developmental constraint, or they may prefer to give the concept of evolvability a formative role. (Their approach is of course shaped by many other concepts as well.) This is an example of a choice that was open in the recent past, but that may be closing in favour of evolvability, or perhaps mutating into a new way to shape the approach of evolutionary biologists.[31]

Economists and game theorists may think of their objects of study as entities that optimize, maximize and reach equilibria. Alternatively they may use concepts such as those of aspiration to some level of profit and ϵ-Nash equilibrium, in order to think of their objects of study as merely working toward outcomes that are regarded as good enough.[32]

[29] Winther, “Parts and Theories in Compositional Biology”, pages 479-494.

[30] Artmann, “Biological Information”; Griffiths and Stotz, *Genetics and Philosophy: An Introduction*, chapter 6.

[31] Hendrikse, Parsons and Hallgrímsson, “Evolvability as the Proper Focus of Evolutionary Developmental Biology”, argues for the importance of evolvability. For a nuanced view of the shift to evolvability and of that concept’s functions within the discipline see Brigandt, “From Developmental Constraint to Evolvability: How Concepts Figure in Explanation and Disciplinary Identity”. For a survey of the field see Larson, “Concepts in Character Macroevolution: Adaptation, Homology, and Evolvability”.

[32] Thinking in terms of aspiration can expose surprising consequences, such as a tendency to collusion: Dixon, “Keeping up with

Those who work on international relations may take on the approach of world-systems theory, and base their work on the concept of a single worldwide economic system within which states have various roles. Alternatively they may base their work on the concept of the individual state and its abilities, and refer to an overall system to explain how those abilities may be limited.[33]

Historians may choose to use the concept of class struggle to shape their approach to particular periods of history. The existence of a choice as to whether to do so in at least some historical work can be demonstrated most strikingly by noting that the concept has been used to shape a study of ancient Greek history, while that period is also often studied without recourse to the concept.[34]

Suppose that the use of certain concepts to play formative roles is optional. Then a choice of some concepts rather than others will guide researchers to develop their discipline in certain ways rather than other equally feasible ways, and to reach conclusions of certain types rather than other types. We need to explore the consequences for our confidence. We shall concentrate on this question of effects on confidence, and shall leave to one side the question of motives for choosing particular concepts. One very strong motive can be that the choice of new concepts opens up whole new vistas of study, as for example when new developments in anthropology allowed the history of witchcraft to be studied in new ways, or when modern economic theory allowed

the Joneses: Competition and the Evolution of Collusion". On ϵ-Nash equilibria see Shoham and Leyton-Brown, *Multiagent Systems: Algorithmic, Game-Theoretic, and Logical Foundations*, section 3.4.7.

33 Wendt, "The Agent-Structure Problem in International Relations Theory", introduction and sections 1 and 2.

34 De Ste. Croix, *The Class Struggle in the Ancient Greek World: from the Archaic Age to the Arab Conquests*.

better studies of the history of industrialization.[35] We shall however make some remarks on the advantages of there being a range of different approaches within disciplines in section 6.3.

We shall first look at the extent of choice in the natural sciences and implications for our confidence, before moving on to the social sciences and the humanities. We shall leave mathematics to one side. Mathematics is, as so often, a special case because it is not tied to the nature of the physical world. Mathematicians can in principle define whatever entities they choose, so long as the definitions do not allow contradictions to be deduced. They will however find themselves more constrained if their task is to develop mathematical tools that will be useful in the natural sciences.

6.2.3 The natural sciences

While there are sometimes choices to be made in the natural sciences, this is not the norm. On the whole, there are certain concepts that work and others that do not. We shall now explore reasons for this lack of choice.

We must start by considering choices of accounts of topics. Once we have considered choices of accounts, we shall consider choices of concepts to play formative roles.

6.2.3.1 Choices of accounts

In the natural sciences, the accounts of primary interest are those that make general claims, rather than giving

[35] Evans, *In Defence of History*, page 83.

accounts of events at particular times and places. In our discussion here, "accounts" will mean accounts that make general claims, and particularly accounts that are elaborate enough to amount to theories.

Researchers in the natural sciences are generally delighted to have little or no choice of accounts of given topics. They are keen to identify the single correct account of each topic from among rival accounts. This goal may seem out of reach when controversies endure for years. And researchers can never be sure that currently accepted accounts will continue to hold sway against all contenders, even though it may turn out that they do hold sway permanently. But they still pursue the goal assiduously.

This does not mean that researchers must seek to eliminate all but one of several accounts that formulate their claims about the world in different terms without conflicting with one another.[36] The use of different formulations may help people to understand what the accounts say. A range of formulations may also help to suggest a wider range of new lines of enquiry than a single formulation would suggest. Researchers do sometimes eliminate formulations, either because they are too cumbersome to be useful or because they lack explanatory power, but there is no general rule that all but one of several non-conflicting formulations must be eliminated. Researchers should however aim to eliminate all but one of several conflicting accounts, whether they predict different phenomena or conflict at some conceptual level.

[36] There are for example several different formulations of quantum mechanics that can be used: Styer et al., "Nine Formulations of Quantum Mechanics".

6.2.3.2 Choices of concepts

When concepts play formative roles, they influence the ways in which researchers think. This means that if there is a choice of concepts to play formative roles, there will be choices of accounts. Some of those choices might well be between accounts that conflicted with one another. We must therefore consider reasons why there might be choices between conflicting accounts.

One reason why there might be choices of accounts is that the evidence might be inadequate to decide between accounts that made different claims about phenomena. For example, there are rival non-supersymmetric and supersymmetric theories in physics that cannot be tested until particle colliders of sufficient power are built.[37] A concept that plays a formative role in the supersymmetric approach but not in the non-supersymmetric approach is the concept of superspace.

Faced with a lack of evidence to decide such questions, our confidence in accepted claims would naturally be reduced. But researchers should themselves hold back, and should not assent to the dubious claims without more evidence. So long as they held back, the question of our confidence would not arise.

Our worries should however go wider than that. If different choices of concepts to be given formative roles would lead to conflicting theories, between which it would not

[37] Lykken and Spiropulu, "Supersymmetry and the Crisis in Physics". At the time of writing the question has not been finally settled, but progress has been made and more progress may be expected. The example has the special feature that supersymmetry is not tested in its full generality. Bounds are set on the masses of superpartners in order to make supersymmetry falsifiable given the methods in use.

currently be possible to decide, we might reasonably be concerned that formative use of any of the concepts would be dangerous, even in other areas where no conflicting theories were in contention. If some concepts were in danger of being ruled out of their formative roles once more evidence became available in the areas in which there were conflicting theories because formative use of the concepts lent too much support to theories that had to be discarded, we would have to contemplate the possibility that any or all of the concepts currently in formative roles would have to be displaced from those roles. This risk would place in doubt the status of work elsewhere that reflected formative use of those concepts.

Another reason why there might be choices of accounts is that the natures of the evidence, of possible accounts and of the links between evidence and accounts might be such that there was automatically scope to choose concepts to play formative roles and then use them to give accounts that handled the evidence in different ways. We would be back to the concern about the underdetermination of theory by data that we discussed in section 6.1.1.2. But as we noted there, underdetermination may not be widespread.

We may add that close inspection of some examples of choices of concepts shows that a combination of the accumulation of evidence and careful attention to how it is best handled can leave less scope for choices of concepts to be given formative roles than one might have expected, even in natural sciences that are reasonably high up the scale. One good example is that of levels of selection in evolution. Researchers are arguably not free to give different accounts based on the different formative concepts of genic, individual and group selection.[38] Another example is that

[38] Okasha, *Evolution and the Levels of Selection*, especially sections 4.3, 5.5.1 and 5.7.

of motivation. There are many concepts of motivation at the disposal of those who formulate theories of how brains guide behaviour, but the concepts are not all on a par.[39]

6.2.4 Social sciences and humanities

We shall now consider why choices of formative concepts are wider high up the scale of disciplines than low down the scale, before considering some constraints on choices and some implications for our confidence.

6.2.4.1 The scope for choice

As we noted in section 6.2.3.2, a choice of formative concepts requires scope to give different accounts of the same topics. We must therefore start by considering that scope.

In the social sciences and the humanities, it is perfectly possible for several different accounts of a topic to remain in play indefinitely. They may well not conflict with one another. Then researchers need not feel any pressure to decide between them. Rather, there may be a range of accounts that can all be endorsed and that in combination can give a fuller understanding than any one of them could on its own – a possibility that we noted in section 2.1.2.1.

One way to arrive at different accounts of a topic is to accord formative roles to different concepts. One example is the use of different concepts of neighbourhoods and of processes of their change in sociological analyses.[40] Another example

39 Berridge, "Motivation Concepts in Behavioral Neuroscience".
40 Schwirian, "Models of Neighborhood Change".

is the choice of whether to use Marxist concepts, such as those of language as practical consciousness and social class, in the analysis of periods of history. Their use can have a marked influence on both the selection of evidence and the fashioning of accounts.[41]

This freedom to have several accounts, formulated in ways that reflect decisions to accord formative roles to different concepts, does not spring directly from lack of evidence. Rather, it springs from the natures of higher disciplines.

We remarked in section 5.3.2 that mechanisms that may be identified in the social sciences and the humanities are such that it is not feasible even to gesture at intelligible relationships between those mechanisms and what fundamental physics would say went on in the world. We may extend this point beyond the identification of mechanisms to other types of description of the world. Descriptions given in the social sciences and the humanities are not to be translated into physical descriptions, except when description is limited to matters such as the physical locations of people and objects, the flows of rivers and the energy needs of factories.

It is therefore no surprise that any sense that there might be a single right way to describe the world in physical terms should not feed through into a sense that there might be a single right way to describe the world in the terms of any of the social sciences or the humanities. The claims of physics, chemistry and biology must not be contradicted in the social sciences or the humanities, but that is not much of a constraint.

[41] Blackledge, *Reflections on the Marxist Theory of History*, chapter 1.

Nor is the evidence as viewed by researchers in higher disciplines a particularly independent constraint on what those researchers conclude. The reading of it is all too easily shaped by the approaches that researchers choose. Evidence is read in the light of theory in all disciplines, but the constraints on possible readings are tighter in lower disciplines because pervasive claims narrow the acceptable ranges of readings. For example, physics in general can tell physicists in narrow fields how their instruments must be regarded as working, and can insist that they draw certain conclusions from what their instruments tell them.

Finally, the lack of expectation of translatability into physical descriptions confers freedom to devise concepts that would have little or no prospect of being related easily to physical concepts, and this will broaden the range of concepts that may be chosen. Researchers may prefer concepts that can be related to the concepts of biology in order to give accounts that explain by relating human ways of life to human biology, and they may prefer such concepts even without being sociobiologists, but they do not have to prefer such concepts. Such a preference can easily be outweighed by a preference for concepts that make it easy to give explanatory accounts within the terms of the social sciences or the humanities.

Although the freedom to keep several accounts in play does not spring directly from lack of evidence, there is an indirect connection. In historical disciplines, evidence is patchy because of the state of the record. In those social sciences in which surveys are conducted and data are collected to order, it is not easy to be sure that the evidence obtained is exactly what it would need to be in order to answer questions decisively. This is partly because of the proneness of human objects of study to act for reasons that are hard to discern, and partly because the most interesting questions

for researchers are often not ones that are easily formulated in ways that would allow researchers to say which results from surveys would imply which answers to those questions. Disciplines have developed to allow themselves to thrive despite such difficulties. Claims are debated in ways that do not rely solely on tests of the kind that would be used in the natural sciences. Nonetheless, the fact that encounters with evidence are less forceful in higher disciplines than in lower ones contributes to greater freedom to have a range of accounts in play, and hence to greater freedom to choose concepts to play formative roles.

6.2.4.2 Constraints on choices

While researchers in the social sciences and the humanities may have choices as to which concepts play formative roles, their choices are constrained. A choice must lead researchers to formulate coherent sets of accounts of different topics. And if the relevant discipline is concerned with the conduct of individuals, the choice must allow researchers to draw on a psycho-social understanding.

The need for coherence

Individual accounts of different topics must not only be coherent within themselves. They must also cohere with one another. This requirement is stronger than a requirement to avoid outright contradiction, and it constrains choices of concepts to play formative roles.

Choices are constrained because a choice of concepts should make it possible to give accounts of different topics within a discipline, such that those accounts would form a coherent set. The accounts should fit together in a structure, or

overlap in a patchwork, so that between them they give a set of accounts of a wide range of phenomena which has at least some degree of unity. Moreover, the way in which the accounts work together should itself reveal something about the world and should invite researchers to make new connections. For example, in economics, accounts of supply and demand at the level of individual products and accounts of labour markets and capital markets fit together to explain how businesses may come into being, prosper, or fail, and an understanding of all of these things can contribute to a theory of whole economies and of the effects of government policies. Not all concepts that might play formative roles would allow researchers to give coherent sets of accounts in a satisfactorily straightforward way. For example, a Marxist concept of the values of products as given by the labour used to make them makes it difficult to give a coherent view of the economy. Defences have been offered, but they are suspiciously convoluted.[42]

Psycho-social understandings

If a discipline is concerned with human conduct at the level of identified individuals, the concepts that researchers use must allow them to draw on a psycho-social understanding. Human beings, when considered as thinkers and agents, are so complicated that it is necessary to draw on such an understanding in order to have any hope of making sense of their conduct. Concepts that play formative roles therefore need to bear reasonably straightforward relationships to the concepts that are used in everyday life.

[42] Examples of defences can be found in Freeman, Kliman and Wells (eds.), *The New Value Controversy and the Foundations of Economics*.

The point does not apply directly to the statistical study of aggregates of human beings, but description in terms that allow connections to be made to the conduct of individuals, as conceived by applying psycho-social understandings, is still important. It matters because it is very helpful if mechanisms can be proposed to explain how results for aggregates arise, and the identification of mechanisms is likely to rely on an understanding of how typical individuals (and some atypical individuals) would conduct themselves in given circumstances.

The concepts that researchers use can most easily allow them to draw on a psycho-social understanding if they are concepts used in everyday life or refinements of such concepts. This is indeed what we often, but not always, see. Historical accounts are shot through with references to the motives of people and with explanations of how the pressures under which they lived and worked influenced their decisions. Sociology builds on everyday concepts such as those of communication, isolation, the family and ritual, all of which play formative roles by setting out some basic categories that can be used to make sense of human conduct. Economics starts with the familiar idea that people have desires and act so as to fulfil those desires – again, an idea so fundamental that it shapes our everyday understanding of human conduct. That basic idea then recurs all over the discipline, even when the analysis gets decidedly technical. For example, a discussion of financial markets can explain the existence and the pricing of different instruments, and the existence of various portfolios, in terms of people's desires for financial reward, inclinations to seize opportunities, and concerns about uncertainty.[43]

[43] Bailey, *The Economics of Financial Markets*, especially chapters 1, 4 and 5.

Having said all that, it is perfectly possible for technical concepts that are not derived from everyday psycho-social understandings to be helpful and even necessary, both when giving accounts of individuals and when giving accounts of aggregates. The concepts of a status set, of ethnocentrism and of cultural capital are examples.[44] Once the contents of such concepts are given, it is often easy to describe the corresponding thoughts and conduct of people in everyday terms, but the concepts are not themselves borrowed from everyday life.

Beyond such concepts, there are two more technical types of concept. The first type comprises concepts that relate directly to the conduct of individuals or aggregates, but are not to be explained in everyday terms without first explaining a considerable theoretical background. Economics abounds in concepts of this type.[45] The second type comprises concepts that relate rather less directly to the conduct of specific individuals or aggregates, but that nonetheless shape the approach of researchers. An example is provided by the twin concepts of structure and agency in the social sciences.

6.2.4.3 Implications for our confidence

The phenomenon of different ways to approach the objects of study of a discipline, ways that are tied to choices of concepts to play formative roles, must make us concerned about the correctness of accepted claims. It may be that a claim will only have visible support if one adopts a certain

[44] Examples of technical concepts can easily be gleaned from the glossaries of textbooks, for example the glossary in Giddens and Sutton, *Sociology*, pages 1051-1074. The examples given here come from that source.

[45] See for example the glossary in Lipsey and Chrystal, *Economics*, pages 697-715.

approach to the objects of study. Other approaches may render the support invisible.

The extent of choice of concepts to play formative roles in higher disciplines is not negligible. To that extent, our confidence may be reduced. But choice is not unlimited. In particular, researchers are constrained by the need for coherence and the need to be able to make use of a psycho-social understanding.

We may conclude that while we should be aware that there are choices of formative concepts, this need not have a serious adverse effect on our confidence.

6.3 Pluralism

The question of choice of formative concepts shades into the question of pluralism more generally. "Pluralism" is an umbrella term for a range of lines of thought in the philosophy of academic disciplines. These lines of thought variously argue that the existence of choices of approach and of claims to make is inevitable, that it is beneficial because the challenge of answering the advocates of other choices is a route to progress, or that it is beneficial because an ensemble of accounts, and in particular an ensemble of explanations, confers greater understanding of the world than a single account or a single explanation.[46]

We shall concentrate on the extent of the benefits of pluralism. We shall also limit ourselves to pluralism in the sense of there being a range of different approaches within a

[46] For some positions that can be brought under the umbrella of pluralism and some issues that arise see Kellert, Longino and Waters (eds.), *Scientific Pluralism*.

given discipline, and there being different claims that may be made when different approaches are used. It may be convenient to see the availability of different approaches as arising out of the availability of choices as to which concepts to give formative roles, but this is not necessary. It may be more straightforward to specify approaches simply by describing methods.[47] And advocates of some claims may oppose other claims either because they think the other claims are mistaken, or because they think those other claims miss the point.

We shall give the notion of different approaches generous scope. Differences of fundamental approach, such as the difference between approaches to social sciences that derive inspiration from the natural sciences and those that emphasize social construction and interpretation, come within our scope. But we shall not set the bounds of the notion of pluralism any wider than this. In particular, we shall not consider whether there is scope for the happy long-term coexistence of claims that contradict one another. Nor shall we slide from pluralism to relativism in any other way.

Those who argue that the existence of a contest between approaches or between claims is beneficial because the challenge of answering the advocates of other choices is a route to progress are clearly correct.[48] A related consider-

[47] Moses and Knutsen, *Ways of Knowing: Competing Methodologies in Social and Political Research*, chapter 1, makes a case for a pluralism of sets of methods. The authors call these sets methodologies, but they also make a connection with formative concepts on page 1 by drawing attention to how choices of methods are influenced by how researchers understand the world.

[48] For a detailed study of how debates can advance toward consensus, the benefits of debate and the conditions for those benefits to be realized in full see Betz, *Debate Dynamics: How Controversy Improves our Beliefs*. For a case study that shows how disagreement can be productive see de Cruz and de Smedt, "The Value of Epistemic Disagreement in Scientific Practice: The Case of *Homo floresiensis*".

ation is that it can be useful to keep in play approaches and views that may have value, and that may uncover difficulties with the current consensus, even when those approaches and views are rejected by most researchers. A healthy contest in which there may be challenges to claims or to the routes by which claims were reached, or even an environment in which non-mainstream views remain in play, can only give us reassurance about claims that survive and remain mainstream. There is an argument that pluralism is particularly important in relation to policy work that may have economic implications, so that economic interests may bias the work, and in relation to work that may have commercial applications, so that full details of the work are not made public.[49] More broadly, contests between views open the way to the formation of rival factions of researchers, the existence of which David Hull has argued to be an important facilitator of the development of science.[50]

Having made this case for pluralism, we should add that challenges to established claims must be well-grounded. There is no merit in attacking claims by proposing approaches or claims that no competent researcher would take seriously. There is such a thing as failure to understand,

The authors note that disagreement can encourage a search for new evidence, can make researchers think again about the evidence they already have and about their assumptions, and can reduce confirmation bias (section 5). In another paper, the same authors add that disagreement helps to counteract systematic bias, such as bias induced by inherited theories, and to promote progress: de Cruz and de Smedt, “Evolved Cognitive Biases and the Epistemic Status of Scientific Beliefs”, section 6. Their modelling of the effects of disagreement has however been criticized as too optimistic: Vaesen and Houkes, “Modelling the Truth of Scientific Beliefs with Cultural Evolutionary Theory”.

[49] Lefevere and Schliesser, “Private Epistemic Virtue, Public Vices: Moral Responsibility in the Policy Sciences”, section 14.3.

[50] Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*, page 26.

and this failure is sometimes exhibited by external critics. We should also note that the benefits mentioned in the preceding paragraph arise out of there being a contest. They do not arise directly out of there being a choice to be made, even though the fact that there is a choice is what gives rise to a contest. While it is healthy to challenge approaches and claims, researchers should not pretend that options are wider than they really are, merely in order to generate an artificial contest.

Those who argue that the existence of choices of approach or of claims to make is beneficial because an ensemble of accounts can confer greater understanding of the world than a single account also have a point. It is however most likely to be a strong point from biology upward on the scale of disciplines. Lower down the scale researchers tend to have few things to say about each object of study, and they therefore have the scope to arrive at single comprehensive accounts. From certain types of biology upward on the scale, the complicated nature of the objects of study makes it impossible to give single comprehensive accounts. Researchers need ensembles of accounts in order to capture enough of the detail of the objects of study, but to do so in chunks that are individually manageable and comprehensible. They may also need ensembles of accounts in order to answer questions that have been posed in particular ways. For example, a question may be posed about the evolution of some feature of some given animal. The fact that the question has been posed in that way may require a response at the level of organisms, but the response may then need to be filled out both by reference to processes at smaller-scale levels and by reference to demands made by the animal's environment.

Even when ensembles are needed, the integration of different accounts is a worthwhile goal, and a goal that

may be perfectly achievable given an appropriate concept of integration.[51] Aspirations to integration, and even more to unification, must however be tempered by the need to take account of the different presuppositions and scopes of different approaches to the same topic, and the different questions that are posed under different approaches.[52] As we noted in section 2.1.2.1, in higher disciplines it is likely to be appropriate to see several accounts as all helping to fill out a picture, rather than seeing them as presenting different facets of some fully integrated single account.

The mere fact that different questions can be asked about the same phenomenon can itself lead to the production of an ensemble of accounts, with all of them providing helpful context for each one. For example, there are different ways to ask why the French Revolution occurred: why did it occur in 1789 rather than 1750, why did revolutions of that general type occur, and why did it occur given the conditions that prevailed in 1789? The first two questions call for explanatory accounts in terms of large-scale social, economic and political features of societies, while the last one calls for an explanatory account in terms of which individuals took which actions and when (although such an account could also be offered as a supplement to the main account in response to the first question).[53]

[51] Mitchell and Dietrich, "Integration without Unification: An Argument for Pluralism in the Biological Sciences"; Mitchell, *Unsimple Truths: Science, Complexity, and Policy*, chapter 6; Brigandt, "Beyond Reduction and Pluralism: Toward an Epistemology of Explanatory Integration in Biology".

[52] See for example Longino, "Theoretical Pluralism and the Scientific Study of Behavior".

[53] Van Bouwel and Weber, "A Pragmatist Defense of Non-Relativistic Explanatory Pluralism in History and Social Science", pages 179-181.

In relation to our confidence, the main thing to be said about ensembles is that their use may deepen researchers' understanding and may thereby allow them to subject individual claims to additional scrutiny. Researchers can ask whether an individual claim makes sense in all of the contexts provided by the different accounts that make up an ensemble. If a claim survives such a test, that provides some support for it. And failure would rightly deter researchers from assenting to the claim.

Some philosophers of pluralism are unduly positive about the existence of rival claims or rival approaches, and come close to seeing a lack of agreement as good in itself. Some think that we should extend to academic disciplines Chantal Mouffe's thought that if democracy is to thrive, contest must be embraced in the form of "agonistic pluralism", rather than being eliminated.[54] Our interest in correctness, as set out in section 1.1.2, means we cannot support the idea that it would be unfortunate to reach agreement on specific questions of approach or on which claims to make, so that erstwhile options came to be excluded, even though we recognize that contests have their benefits.

Our interest in correctness and our inability to see agreement as unfortunate also lead us to have reservations about some aspects of the strong case that Hasok Chang makes for pluralism, despite the fact that his case sets out many undeniable advantages and despite the fact that he demarcates pluralism sharply from relativism. The reservations spring from the fact that his "active normative epistemic pluralism" is a pluralism of "systems of practice". That would appear to be harmless, but what he says about

[54] Mouffe, *The Democratic Paradox*, pages 98-105. For a discussion of the benefits of an extension to academic disciplines see van Bouwel, "The Problem With(out) Consensus: The Scientific Consensus, Deliberative Democracy and Agonistic Pluralism", pages 135-137.

the history of work on the chemical composition of water leads us to think that keeping systems of practice alive to the extent that he envisages would involve retaining as options (although not as options that anyone should choose) substantive claims which are not merely out of fashion for the time being, but should plainly be discarded for ever.[55]

We shall return to the usefulness of ranges of views in section 9.2.4.

[55] Chang makes his case in *Is Water H_2O? Evidence, Realism and Pluralism*, chapter 5. He demarcates pluralism from relativism in section 5.1.3, explains his pluralism's name in section 5.2.1, and presents it as a pluralism of systems of practice in section 5.1.2.

Chapter 7

Experiments and Sources

7.1 The distinction

We must distinguish between experiments that researchers set up in order to yield evidence, and sources that provide evidence but were not created to do so. Some disciplines, particularly within the natural sciences, rely heavily on experiments. In the humanities, sources are far more significant than experiments. The social sciences are an intermediate case in which surveys have special relevance – and we shall have reason to regard survey data as similar to sources. But disciplines do sometimes obtain evidence in ways that are more usually associated with other disciplines. Sources can play roles in the natural sciences, for example when the fossil record is used in evolutionary biology. And experiments are used in the social sciences.

An experiment can be devised to answer a particular question, or to gather evidence for some other well-specified purpose. If an experiment is well-designed, the experimenter can expect that the precise question posed will be answered, or that the evidence gathered will be well-suited to the purpose. (Not all experiments give direct answers to particular questions, such as the question of whether the occurrence of one phenomenon causes the occurrence of another. The world as studied by disciplines from biology upward can easily be too complex for that, and in particular it can be impossible to separate out the contributions of a number of different causes to the production of some effect.[1] There are also, in all experimental disciplines, experiments that are conducted in order to explore phenomena before formulating any claims that could be tested.[2])

By contrast, sources must be taken as they are. They were created for purposes other than current research, and they may not be well-suited to answering researchers' questions. Researchers who rely on sources must ask whether they have understood the evidence correctly. The state of the evidence may have been influenced by many factors other than the ones that interest them, including factors of which they are unaware.

This is not to say that sources are always a dubious form of evidence. Some disciplines, such as history, are built around their use and are therefore perfectly adapted to their limitations. Even when that is not so, sources can be of great value. And it is sometimes possible to see processes that have led to the generation of source data as having some of the qualities of well-designed experiments. It may be possible to say that certain effects resulted from

[1] Mitchell, *Unsimple Truths: Science, Complexity, and Policy*, chapter 4.

[2] Arabatzis, "Experiment", pages 197-198.

specific changes in the environment, either because the environment was otherwise almost completely stable, or because it is possible to analyse data in ways that in effect retrospectively create control groups.[3]

Survey data may be obtained for current purposes, but they are generally obtained in environments that are not controlled by experimenters. We shall therefore treat the use of survey data under the heading of sources, in section 7.3.2.

The considerations that may lead us to have special concerns about the use of sources also apply, to a lesser extent, when researchers use experimental results that were obtained for purposes other than current ones. Such results may have been obtained under carefully controlled conditions, and awareness of precisely how they were obtained certainly assists in their interpretation, so difficulties are on the whole less than they would be with sources derived directly from the uncontrolled world. But there may still be concerns about influences that may have been irrelevant to the purposes of the original experimenters but that may be undesirable in the context of current re-use of the results. It is therefore sometimes appropriate to regard secondhand experimental results as sources.

7.2 Experiments

Experiments are standard in most disciplines low down the scale, but they can also be used reasonably high up the scale. For example, although evolutionary biology depends heavily on pre-existing sources, it can also use experiments on organisms that run through generations

[3] Morgan, "Nature's Experiments and Natural Experiments in the Social Sciences".

rapidly enough to allow evolution to be observed in the laboratory.[4] Evolutionary biology also draws on results that have been established experimentally in other areas of biology and in chemistry. Moving up into the social sciences, political science now makes considerable use of experiments in which people are assigned to groups, and are then subjected to various stimuli to measure their responses or are asked various questions.[5]

Experiments do not need to have been performed for the purposes of a current piece of work. If results derive from earlier experiments that were conducted according to modern standards they may be used, although there is the point mentioned above that it may be appropriate to treat such secondhand results as if they were sources. If on the other hand earlier experiments were conducted according to standards that would now be regarded as lax, their results may have to be ignored altogether. There is a grey area of results obtained from experiments that would not now be regarded as entirely satisfactory, but that were not so badly conducted as to render their results useless.

There are norms that lay down how experiments should be designed and conducted, as we noted in section 2.1.1.2. In the natural sciences in particular, papers that report new research are expected to set out how experiments were conducted, so as to reassure readers that norms were observed. And papers may have to be retracted if it is later found that there was some problem with the ways in which experiments were conducted, whether or not there was deliberate malpractice.[6] There is however reason for

[4] Buckling, Maclean, Brockhurst and Colegrave, "The *Beagle* in a Bottle".

[5] Examples may be found in the *Journal of Experimental Political Science*.

[6] Some examples can be found on the website *Retraction Watch*, http://retractionwatch.com/.

optimism. Only a small fraction of published papers are retracted, and retraction can be for any one of several reasons, not just because of problems with the ways in which experiments were conducted.[7] On the other hand there is an argument that failure to abide by norms can very easily go undetected, for a variety of reasons that are connected with the availability of resources and the sociology of disciplines.[8] And there is evidence that in at least one discipline, psychology, there is quite widespread use of research practices that are at least questionable, whether or not they are fatal to the reliability of results.[9]

In the social sciences, and sometimes in the natural sciences, there are arguments to be had about the extent to which experiments can allow researchers to make claims about the world outside the context of the experiments. The degree of simplification and isolation involved in experiments can be such as to require great care when moving from experimental results to claims about the wider world.[10] Doubts about whether enough care has been taken may limit our confidence in claims about the world that come to be accepted on the basis of experiments.

Given that there are risks associated with experiments, we may only have full confidence in claims when enough work has been done to lay to rest any doubts about the quality of experiments and about the safety of their interpretation as providing information about the world. Whether this

[7] Grieneisen and Zhang. "A Comprehensive Survey of Retracted Articles from the Scholarly Literature".

[8] Hardwig, "The Role of Trust in Knowledge", section 3; Fanelli, "How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data".

[9] John, Loewenstein and Prelec, "Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling".

[10] For the case of economics see Guala, *The Methodology of Experimental Economics*, chapters 7 to 9.

strict standard is in fact met will depend on the conduct of researchers. We shall consider that issue and related issues in sections 9.4 and 9.6.

7.3 Sources

7.3.1 The interpretation of sources

Sources come in many different forms. They include documents, works of art, and everyday material artefacts. In this section, we shall consider the interpretation of individual sources. We shall include under this heading work on sets of sources, the members of which are compared and contrasted to throw light on one another and to allow overall pictures to be painted, rather than to aggregate information about them in a statistical manner. In section 7.3.2, we shall consider the use of results obtained from systematic surveys of populations.

Issues in the interpretation of individual sources can arise at several levels, from the immediate reading of evidence up to high-level interpretation.[11] Moreover, different theories and principles may need to be brought to bear at different stages of interpretation.[12]

[11] For examples of interpretation at different levels, including a basic level that is very close to sources which might at first glance appear to be quite straightforward to read, such as Roman inscriptions, see Morley, *Writing Ancient History*, pages 69-92.

[12] See for example the discussion of the role of middle-range theories in archaeology in Kosso, *Knowing the Past: Philosophical Issues of History and Archaeology*, chapter 3. (As Kosso explains on pages 61-64, middle-range theories are not theories of middling generality, but theories of any type when they are used to help archaeologists to understand how the archaeological record was formed.)

We must distinguish between sources that were, when produced, intended to convey information to someone, and those that were not intended to do so. For example, written documents were mostly intended to convey information, while items of undecorated pottery were mostly not intended to do so.[13]

7.3.1.1 Sources intended to convey information

We shall now set out some of the complexities that can arise in the interpretation of sources that were intended to convey information. We shall return to this topic in the context of explanation by the identification of attributed meanings in section 8.3.4.

Researchers must discern the intentions of the creator of a source. He or she might have intended to inform, entertain, mislead or convince.[14] Researchers' grasp of the intentions of the creator will depend on their more general understanding of the society within which the source was created, the individual creator, and the people close to him or her.

Researchers must also consider the limitations of the creator. Was he or she in a good position to achieve the intended purpose? For example, if the source gives an account of some events, was the creator well-informed enough to be reliable?

[13] For remarks on the significance of a distinction between historical and archaeological sources that draws a boundary which lies close to our boundary between sources that were and sources that were not intended to convey information see Kosso, *Knowing the Past: Philosophical Issues of History and Archaeology*, pages 29-33.

[14] For an example of the difficulties of working out an author's intentions see Dodd, "Was Thomas Favent a Political Pamphleteer? Faction and Politics in Later Fourteenth-Century London".

The creator of a source will have acted on the basis of his or her understanding of the position of the intended recipients: what they would have already believed, what they might have wanted, how they might have been influenced, and so on. In order to understand what the creator intended, researchers must therefore grasp how the creator understood the intended recipients. And the first step toward discerning the creator's understanding will be to discern the actual position of the intended recipients.

There will also be conventions of writing to consider. The effects of such conventions might have been well-understood at the time, but those effects may not be at all obvious to modern readers. Good examples of some of the difficulties of interpretation that can arise are supplied by letters and by histories written long ago that were, at the time of writing, secondhand reports of events, and that were sometimes written with purposes that would not be shared by modern historians, but that are now among the available sources that are closest to the events.[15]

While complexities of interpretation most obviously arise in connection with old sources, they can also arise when recent sources are used. For example, they arise when historians seek to make use of films, whether the films were made to inform or to entertain. A skilled historian may be able to use a film of any type to glean information about the context in which it was made, as well as using documentary films to obtain information about the events filmed. Some of the complexities, and the potential of film as a source if correctly used, are indicated by the work of Marc Ferro.[16]

[15] Schulte and von Tippelskirch (eds.), *Reading, Interpreting and Historicizing: Letters as Historical Sources*; Given-Wilson, *Chronicles: The Writing of History in Medieval England*, especially chapters 1 and 3; Pitcher, *Writing Ancient History: An Introduction to Classical Historiography*.

[16] Ferro, *Cinéma et Histoire*.

The difficulties created by such complexities are by no means insuperable. In particular, sources are not interpreted in isolation. There are often several sources, perhaps of several different types, that can be used together. The ensemble of sources as a whole can then provide assistance in the interpretation of the sources individually. The complexities are also not news to researchers. The history of hermeneutics, particularly from the time of Schleiermacher onward, attests to the deep thought that has gone into the question of how to interpret sources. But it is important to be aware that the interpretation of sources is not at all straightforward.

7.3.1.2 Other sources

When a source was not intended by its creator to convey information, some of the complexities of interpretation that we have just noted do not arise. But researchers may still need to consider intentions that lay behind the making of artefacts or behind the making of them in certain ways (for example, by decorating them), and how the artefacts were used. Questions of intention and use only fail to arise in connection with natural remains that were neither deliberately created nor deliberately used, either by human beings or by animals with the mental capacity to act deliberately.

While the interpretation of sources that were not intended to convey information is in one way easier, because some or all questions of intention and use do not arise, it is in another way harder. There is no message that is manifestly conveyed by such sources. Researchers have no obvious starting point for their quest to extract information from the sources.

This does not however leave them with no way to begin. The existing corpus is of immense importance. With artefacts, researchers are likely to draw heavily on the corpus to tell them about societies and everyday life at the relevant time and place. With other sources, such as fossils, researchers will have not only the existing record of sources, but also theories that set out how sources arose and what features of them will indicate their dates, their relationships to other sources of the same general type, and so on. There is also the assistance in the interpretation of individual sources that can be obtained from the whole ensemble of sources, which may be of several different types.

Despite such forms of assistance, interpretation can still be difficult. Difficulties are especially great when sources are sparse. Archaeological remains present particular difficulties, partly because of the sparseness of sources and partly because the thoughts that lay behind decisions to create particular artefacts or to use natural resources in particular ways have often left no direct traces. It is not surprising that there has been considerable debate over proper approaches to archaeological work and over the reliability of conclusions that may be reached.[17] When there are debates about how interpretations should be made and about what a discipline can achieve, we may be glad that the issues are exposed rather than ignored. But the very fact that there are such issues may reduce our confidence in accepted claims, unless they enjoy the assent not merely of the members of one school of thought but of the members of all or most schools of thought.

[17] For different approaches to the recovery of cultural meaning from material culture see Hodder and Hutson, *Reading the Past: Current Approaches to Interpretation in Archaeology*. For a broad review of debates over what archaeology can achieve and how the legitimate goals might be achieved see Wylie, "Philosophy of Archaeology; Philosophy in Archaeology".

7.3.1.3 Interpretation to facilitate explanation

We must consider the possibility that sources will be interpreted in certain ways with a view to making it easier to give explanatory accounts.

There is a modest form of interpretation in which clues in the sources are combined with a general understanding of how things are likely to have been, in order to support the addition of details that are not in the sources. The aim is to fill out the context, and thereby allow the sources to be interpreted as supporting a satisfactory narrative. A work of political history, for example, must tell a compelling story of events if it is to be worth reading. The story must be faithful to the evidence, or at least to any evidence the veracity of which there is no good reason to doubt, but historians may need to add details that are not directly supported by specific pieces of evidence in order to complete the story. They do not do anything so crude as asserting that particular meetings took place when there is no direct evidence that they did. Rather, they do things like assert that given that people were in certain situations, they must have thought, intended or discussed certain things. Such claims can play important roles in explaining the course of events. One example is Robin Lane Fox's claim, in relation to the plot against Alexander in 330 BC, that the plotters "must have discussed the future before they acted" and "would have had plans for a replacement". These are worthwhile additions to the narrative, because they support the delineation of a plausible (but not certain) role of Philotas that would help to make sense of the course of events.[18] Another example, in which a desire to make sense of events leads a historian to regard the only direct evidence for an event as misleading, is Edgar Feuchtwanger's view

[18] Lane Fox, *Alexander the Great*, pages 286-288.

that when Bismarck met Wilhelm I in September 1862, he must have already known about Wilhelm's threat to abdicate and must have worked out his approach on that basis.[19]

Such additions of plausible detail can be allowed because it is expected that historical events which are described in terms of the activities of individuals will be explicable by reference to a general understanding of how people tend to think and act. It is an assumption of any everyday understanding of human beings that their conduct is usually intelligible. That assumption is justified by experience. And the addition of plausible details greatly facilitates the giving of explanations. Moreover, reputable writers signal the fact that details are being added by using phrases like "We may suppose that", or "It must have been that". The addition of plausible details must however be tightly controlled. It has been argued that microhistorians, with their focus on the lives, the characters and the thoughts of individuals, can be overly inclined to go beyond the evidence.[20]

A larger-scale form of interpretation takes place when researchers decide to read evidence in the light of some manifestly optional formative concepts, in order to facilitate the giving of explanations. For example, Pierre Bourdieu's analysis of the relationships between patterns of aesthetic taste and patterns of class depends on his use of a notion of capital that is divided into economic and cultural capital. It is important that the latter is regarded as a form of capital, similar to the former in respects such as the scope to inherit

[19] Feuchtwanger, *Bismarck: A Political History*, pages 80-81. The direct evidence in question is the account of the meeting in Bismarck's own reminiscences. Relevant extracts from those reminiscences are given in Steinberg, *Bismarck: A Life*, pages 178-179.

[20] Magnússon and Szijártó, *What is Microhistory? Theory and Practice*, pages 106-110.

it.[21] Another example is provided by an interpretation by Christopher Tilley of Swedish archaeological remains in the light of a social model of power relations, the use of which brings some optional formative concepts into play. Tilley first sets out the model, then analyses the pieces of evidence in their own right, identifying links between bodies of evidence of different types, and finally interprets the evidence in the light of the model. He argues that the identification of links not only supports the chosen interpretation, but is also needed to allow the evidence to be interpreted at all.[22] This role for links between bodies of evidence of different types does create a more subtle structure than one in which evidence is simply interpreted under the influence of some optional formative concepts. If the links were established in a way that was manifestly independent of the choice of formative concepts, the interpretation of the evidence would be a good deal less under the influence of that choice than it would otherwise be. But in practice it is very likely that the identification of such links will be heavily influenced by the choice of formative concepts, and confirmation that the right kinds of link have been identified will come from the successful application of those concepts.

Researchers may make this kind of choice of ways to read evidence, not for the disreputable reason that they want to justify their formative concepts even at the cost of using biased interpretations of evidence, but for the more respectable reason that it allows them to make effective use of their conceptual resources. To read evidence in a

[21] Bourdieu, *La Distinction. Critique sociale du jugement*, especially chapters 1 and 2.

[22] Tilley, "Ideology and the Legitimation of Power in the Middle Neolithic of Southern Sweden". Tilley comments on the relationship between the finding of links and the interpretation of the evidence on page 144.

particular way under the influence of optional formative concepts need not be to distort evidence. It may be to discover good and worthwhile explanations of phenomena by bringing some formative concepts to bear. The reading of evidence in this way does however mean that success in giving explanatory accounts cannot give much in the way of confirmation that the formative concepts really ought to be used.

The risk that sources will be interpreted inappropriately in order to allow explanatory accounts to be given may reduce our confidence. The surest protection is, as so often, critical scrutiny by other researchers. We shall now note some varieties of scrutiny.

7.3.1.4 Scrutiny of the use of sources

Scrutiny will involve checking that researchers investigated the authenticity of sources, dated sources as accurately as possible, examined with particular care sources that were not typical of their supposed times and places of origin, and considered whether sources might have become corrupted in the period since their creation or in the process of extracting them for investigation.

There are other types of scrutiny which relate more to the ways in which researchers use sources to support claims than to whether researchers have made checks on the sources themselves.

One type of scrutiny is to check the extent to which sources can be corroborated. The corroboration may come from other sources of the same general type, for example when there are several apparently independent accounts of the same event. And the lack of such corroboration for accounts

of events that one would expect to have been notorious can cast doubt on slender sources, especially if they are not quite contemporary, are not eye-witness reports, or are hard to reconcile with other evidence. For example, Martin Luther's nailing of his 95 Theses to the church door has been doubted on such grounds.[23] Alternatively, corroboration may come from work of a different type, as when archaeologists experiment with flintknapping in order to support or test readings of remains.[24]

Another type of scrutiny is to challenge specific interpretations by giving alternative ways to read the sources. In particular, the use of written sources that directly encourage certain views of events can be challenged if researchers appear to have assented to the encouraged views too readily.[25] This kind of search for alternative interpretations is the first line of defence against the interpretation of sources in ways that are unjustified but are encouraged by their making it easy to give explanatory accounts, or by their giving support to views that researchers already favour. It does however always require going back to the primary sources. Scrutiny based solely on secondary sources would fail to detect biases that had been incorporated when

[23] Iserloh, *Luthers Thesenanschlag. Tatsache oder Legende?* For the lack of corroboration see pages 19-24. For the limitations of the critical source see pages 17-18. For the difficulty of reconciliation with other evidence, in this case letters Luther wrote to bishops, see page 16. What did or did not happen is still disputed, even though additional evidence has since come to light: Ott and Treu (eds.), *Luthers Thesenanschlag – Faktum oder Fiktion*.

[24] Carr and Bradbury, "Flake Debris and Flintknapping Experimentation". The relationship between work within the discipline being practised (such as archaeology) and information obtained from work done in supporting disciplines (such as the physical and chemical analysis of artefacts) can be complex: Jones, *Archaeological Theory and Scientific Practice*, especially chapters 4 and 5.

[25] An example of such criticism is Kessler, "Power, Not Progress: An Alternative Reading of L'Hôpital's Legal Reforms".

they were written.[26]

Finally, to return to formative concepts, the use of optional formative concepts when interpreting sources should of course be scrutinized. But in addition, the formative concepts themselves should be exposed to criticism. If they are found wanting, explanations that they facilitate may need to be discarded. For example, Bourdieu's concept of cultural capital has been heavily criticized in its role as an explanation of a supposed lack of the social mobility that one might expect to result from an educational system.[27]

7.3.2 Surveys

We shall now turn to issues that relate to the systematic collection and analysis of data from a population. These issues arise in connection with field research, such as the observation of animals in the wild or the use of sociological surveys. They also arise in work in the humanities, such as the analysis of a wide range of texts in order to identify trends in the development of the relevant language, or the use of surveys of people's views on carefully constructed scenarios in order to put philosophers' intuitions to the test.[28]

[26] Some of the dangers of doing research on the strength of secondary sources are set out in Lustick, "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias". Lustick writes with reference to the original carrying out of research, but the dangers he identifies extend to the scrutiny of research that has been done by others.

[27] Goldthorpe, "'Cultural Capital': Some Critical Observations".

[28] For an example of the analysis of texts see Acerbi, Lampos, Garnett and Bentley, "The Expression of Emotions in 20th Century Books". For surveys in philosophy see Alexander, *Experimental Philosophy: An Introduction*.

It might seem that a survey was a form of experiment, one that could determine the values of some variables in a population in a way that could answer a precise question because the questions put to the population could be chosen to serve the researchers' purposes. (We include in the category of surveys both surveys of the views of human beings and surveys of animals, plants, geological formations and so on where questions are put to members of the population not by speaking to them, but by taking measurements in other ways.) But when we are concerned with the confidence we may have in claims that are based on sets of data obtained from surveys, it is appropriate for us to regard those sets of data as closer to sources than to the results of experiments. The use of pre-existing populations with their own characteristics means that researchers must be less confident that responses to interrogation have the significance they would need to have in order to answer the researchers' questions than they might be if data had been collected in artificially created experimental environments.[29]

We must also distinguish primary data, collected by researchers for their specific purposes, from secondary data, taken from established databases. (Work in a third category, literature that reports researchers' interpretations, is not a source of data at all, except to the extent that it contains reports of data that can be read in isolation from the interpretations.) Secondary data may have been collected carefully, following well-designed procedures that researchers can study to confirm that the data should be suitable for their purposes. Established databases may include far more data than individual research teams could

[29] For a discussion of how the uncontrolled nature of pre-existing populations can make it difficult to reach conclusions about causes and effects see Przeworski, "Is the Science of Comparative Politics Possible?".

collect. But there remains the question of whether data taken from a database that has been compiled to suit a wide variety of purposes will be ideally suited to the purposes of a specific project.[30] It is not only the way in which basic data were recorded that may give rise to concern. The ways in which items of data have been categorized and linked in the database may fail to represent some members of the relevant population or the relationships between them adequately.[31] This may lead to errors when a survey involves picking out members with certain characteristics and studying them.

Finally, not all data are of types that facilitate statistical analysis. In particular, human responses to questions that are not forced into the mould of choices between prescribed options can be difficult to analyse.

7.3.2.1 The design and conduct of research

Projects to collect and analyse data must be designed and conducted carefully. In surveys of people, questions to put to the population must be devised so as to elicit the required information, they must be written and used in ways that will not introduce bias, and results must be collected in ways that will lead to accurate reports. When data are obtained by surveying a population, whether of people or other entities, without speaking to its members, comparable precautions must be taken. In either case, analyses must be conducted in ways that impose strict requirements on what must be shown in order to draw conclusions. Methods of analysis should also, so far as possible, negate the effects of

[30] For some issues that arise with the use of secondary data see Schutt, *Investigating the Social World: The Process and Practice of Research*, chapter 14; St. Martin and Pavlovskaya, "Secondary Data".

[31] Brown and Simpson, "The Curious Identity of Michael Field and its Implications for Humanities Research with the Semantic Web".

any biases that may have been introduced at earlier stages despite all the precautions that were taken.

We can gain reassurance from the fact that disciplines which use surveys have well-developed methods, the use of which helps to ensure that work is done properly. Methods to use are set out in comprehensive manuals.[32] In addition, there are special methods that can help researchers who face special challenges.[33] If we can see that established methods have been used, that should increase our confidence in the claims made.

7.3.2.2 Data mining

Data mining is a relatively new way to use survey results or other sources of substantial quantities of data. Researchers start by identifying some entity, such as an ecosystem, an economy or a political community, or some source of data, such as a sequence of experiments or a body of literary texts. They then collect and analyse large quantities of data, and use the data to identify patterns that may be regarded as significant and worthy of further investigation, to develop models of the parts of the world that gave rise to the data, or to test hypotheses. Some very sophisticated techniques have been developed, and the computational resources that are needed to make full use

[32] Examples are Coe (ed.), *Geological Field Techniques*; Karban, Huntzinger and Pearse, *How To Do Ecology: A Concise Handbook*; Bryman, *Social Research Methods*.

[33] For an example of a method to ensure that parts of a population are not accidentally excluded from a sample see Landry and Shen, “Reaching Migrants in Survey Research: The Use of the Global Positioning System to Reduce Coverage Bias in China”. For examples of methods to perform analyses when samples may be statistically unsatisfactory see Fu, Winship and Mare, “Sample Selection Bias Models”.

of those techniques, including not only programs that find patterns but also data visualization software and workflow management systems, are now readily available.[34] Much of the development of data mining techniques has been driven by the desire of commercial enterprises to predict which people would be potential customers, who would be likely to repay loans on time, and so on, rather than by the needs of academia. Textbooks in particular often reflect such commercial concerns. But even though the ultimate objectives of commercial work differ from those of academic work, they have the same intermediate objectives of identifying patterns and testing hypotheses.

When this kind of work is done, we may have concerns about the degree of support that is enjoyed by the claims that result. The main concern is that patterns found may not really support claims that are made, and that models developed may not appropriately represent the world. There are several points to consider.

The first point is this. A large body of data may contain patterns that could support many different conclusions, simply because the body of data is large. It is quite likely that some associations or other will arise by chance. Researchers who go to the data in pursuit of interesting patterns in general are therefore likely to find something or

[34] Textbooks include Nisbet, Elder and Miner, *Handbook of Statistical Analysis and Data Mining Applications*; Tufféry, *Data Mining and Statistics for Decision Making*. One can gain an impression of the possibilities by browsing the journal *Data Mining and Knowledge Discovery*. For a discussion of data mining in ecology see Hochachka et al., "Data-Mining Discovery of Pattern and Process in Ecological Systems". For examples of the use of data mining in some other disciplines see Monroe, Colaresi and Quinn, "Fightin' Words: Lexical Feature Selection and Evaluation for Identifying the Content of Political Conflict"; Pumfrey, Rayson and Mariani, "Experiments in 17th Century English: Manual versus Automatic Conceptual History".

other, even if there is no good reason to regard what they find as significant.[35]

The second point is this. Initial inspection of data may disclose some pattern that suggests a likely conclusion or a likely model. Researchers may be inclined to look favourably on that conclusion or model before examining its level of support, and may then concentrate on the support it gets from the data while disregarding any lack of evidence that the pattern is really significant. We therefore need to pay attention to how researchers initially approach data. Tools of visualization that help researchers to spot patterns are in common use.[36] It is also possible to give the researcher's eye for an interesting pattern an express role not merely at the start, but in the progressive exploration of the data in pursuit of ever more sophisticated and interesting conclusions.[37] But the fact that a pattern strikes researchers as interesting does not show that it is of any real significance.

The third point is that we must be concerned about the ways in which hypotheses are tested when data have suggested the hypotheses. It is generally unsound to use the data that suggest a hypothesis to test it, although there are special methods that can sometimes be used.[38] It is therefore important to regard hypotheses as tested only if they have been tested on independent data or appropriate corrections have been made. Claims that cannot be tested

[35] Smith and Ebrahim, "Data Dredging, Bias, or Confounding: They Can All Get You into the BMJ and the Friday Papers".

[36] Fox and Hendler, "Changing the Equation on Scientific Data Visualization".

[37] Yu, Yurovsky and Xu, "Visual Data Mining: An Exploratory Approach to Analyzing Temporal Patterns of Eye Movements", page 38.

[38] Maxwell and Delaney, *Designing Experiments and Analyzing Data: A Model Comparison Perspective*, pages 200-201 and 213-221.

in this way, perhaps because of a lack of independent data or because the usual corrective measures are inappropriate, may be of interest, but we should not have confidence in them unless alternative precautions have been taken.[39]

There is a general response to our main concern, and also to the other concerns that we shall mention below. This is that users of data mining are aware of its perils.[40] Users are also aware of precautions that can be taken. So when we consider the confidence we should have, we should either look in detail at the precautions taken or, less demandingly but more practically, consider whether the relevant academic climate is such as to enforce the taking of appropriate precautions.

A second concern, closely related to the main concern, is that there are several methods that could be used to process the same data. This may give rise to a suspicion that if different methods had been used, the conclusions drawn might have been markedly different. There is even a worry that incentives to researchers, such as career incentives that make it particularly desirable to obtain exciting new results, may encourage them to choose methods on grounds other than a disinterested desire to advance their understanding of the world. This worry can arise in many types of work, but it is particularly significant in the context

[39] One such precaution is proposed in White, "A Reality Check for Data Snooping".

[40] There is for example a catalogue of leading mistakes in Nisbet, Elder and Miner, *Handbook of Statistical Analysis and Data Mining Applications*, chapter 20. This catalogue is written in the context of data mining with a view to prediction, often for commercial purposes, but the mistakes still translate to more academic contexts, although the descriptions of some of the mistakes would need to be adapted and some additional mistakes would need to be added. For problems that can arise when data mining is used in econometric modelling see du Plessis, "The Miracle of the Septuagint and the Promise of Data Mining in Economics".

of data mining because of the wide choice of methods, the sophisticated nature of analyses, and the scope to decide which data to use and which to exclude.[41] One response to this concern, to give in addition to the general response to concerns noted above, is to point out that the results of computations that use different methods can be compared, with discrepancies performing the valuable role of casting doubt on results or on their significance. Another response is that new methods are introduced in order to make data mining more effective, and that they can be demonstrated to be improvements on old methods in certain circumstances.[42]

A third concern is that if data mining is used to develop a pre-existing model, for example to fix parameters, there is a danger that the choice of the model will reflect inherited prejudice rather than a fresh look at the evidence. There is the general response to concerns noted above, and there is also the more specific response that models can be tested and improved in recognized ways.[43] (This response is relevant to any general concern about claims which are based on models that result from data mining, as well as to this specific concern.) Another specific response is to say that data mining can be used in stages, with an initial model's being very general so that not much prejudice is incorporated, and the model's then being made more specific at each stage in response to data.[44]

[41] Glaeser, "Researcher Incentives and Empirical Methods".

[42] Examples are given in Grimmer, "An Introduction to Bayesian Inference via Variational Approximations"; Stockwell, "Improving Ecological Niche Models by Data Mining Large Environmental Datasets for Surrogate Models".

[43] Nisbet, Elder and Miner, *Handbook of Statistical Analysis and Data Mining Applications*, chapter 13.

[44] Hoover and Perez, "Three Attitudes Towards Data Mining", section 5.

Chapter 8

Routes to Accounts

8.1 Introduction

Accounts and the claims they contain do not drop from the sky. They are created by human beings. In this chapter we shall consider ways in which researchers may work their way from evidence or other starting points to accounts or specific claims. In chapter 9, we shall consider ways in which researchers work, whether independently or with one another, when they produce accounts, make claims and decide whether to assent to claims.

We shall sometimes speak of routes to accounts, and sometimes of routes to the claims that would be embedded in accounts. We shall favour the former way of speaking when the route is one that is most naturally seen as leading primarily to accounts as wholes. We shall favour the latter way of speaking when the route is one that can easily be seen as leading directly to individual claims. The first route we shall consider, the deductive route, is of this latter type. The second route, the good explanation route, is more

naturally seen as taking researchers from evidence to whole accounts.

8.2 The deductive route

Claims are sometimes deduced from evidence or other starting points. We shall call this the deductive route to claims. We shall extend the term to cover arguments that are close to being deductive, as when some of the relevant links between propositions are not quite deductive. We shall do so both because arguments that are close to being deductive can confer almost as much confidence as arguments that are properly deductive, and because some of the issues that arise in relation to use of the deductive route extend to the use of arguments that are close to being deductive.

In mathematics, the starting points are not pieces of evidence obtained from the world but axioms, established results and the contents of established or new definitions. Results must standardly be deduced from these starting points. And relationships between propositions that are not deductive, but are merely almost as strong, are not normally used in argument.

In the natural sciences, the deductive route can play a significant role. For example, data from experiments in physics are used to deduce narrow ranges for the values of fundamental constants, and x-ray diffraction patterns are used to deduce the structures of crystals. But the relevant relationships between propositions will often only approach being deductive in their strength. There may be some modest scope for the starting points of a piece of reasoning to be correct without the conclusion's being correct.

We need to consider concerns in three areas: the quality of evidence, the validity of arguments and the role of the existing corpus. Throughout this section, we shall take it that the relevant arguments for claims have already been set out, and are to be read forward from starting points to conclusions.

8.2.1 The quality of evidence

If claims follow deductively from evidence, and the evidence is not misleading, we should have great confidence in the claims.

Confidence that evidence does not mislead is however a strong requirement. There might be bias in the presentation of evidence which obscured the fact that the correctness of some of the premises of a deduction was not beyond reasonable doubt, especially in higher disciplines where there are plenty of ways to present evidence. And undiscovered evidence, or evidence that had not been studied thoroughly, might have cast doubt on the premises. The higher up the scale we go, the more likely it is that this will be a serious risk. There may very well be a large body of potentially relevant evidence, and it may not have been catalogued and studied systematically.

8.2.2 The validity of arguments

In this section we shall consider the quality of arguments in themselves, rather than the quality of their starting points. Our concern is with validity, not soundness. Our comments will extend to arguments that use relationships between propositions which are not quite deductive. The notion

of validity does not strictly apply to such arguments, but they can still be better or worse. And the controls we shall mention that may detect invalid arguments are also useful as controls over arguments that are not quite deductive.

When a deduction of a claim from evidence conforms to the definition of a proof that is laid down by logicians, with all steps laid out, there is no need to think about controls over the steps in the argument. The argument is simply valid. But even in mathematics, it is very rare for an argument to be set out with such complete formality, with no line being written unless it is an axiom, a theorem, or something that some precise rules of deduction allow given the contents of preceding lines. Proofs are in practice a good deal less formal than that, as we noted in sections 3.5.2.2 and 4.2.1. In the natural sciences, even more informality is to be expected. When there is any degree of informality, it is important to consider whether there are adequate controls over the quality of arguments. Is it likely that invalid arguments, and arguments that fall short of the corresponding standard for work that is not quite deductive, will be disallowed?

We can gain some reassurance from the dialogical conception of deduction. This conception sees a deduction as a dialogue between a proponent who tries to establish a conclusion on the basis of certain premises, and an opponent who tries to show that the premises do not compel the conclusion. We are to imagine a particularly awkward opponent, who is always looking for a way to agree to the premises but reject the conclusion. If the proponent only makes moves that are permitted by the rules of deduction, she will be safe. The opponent will not be able to attack her argument, for example by showing that there are counter-examples to what she claims are deductive steps. But if the proponent makes moves that go beyond what the rules

of deduction permit, an attack will succeed and she will fail to establish the argument's conclusion. So the rules of deduction become rules that determine which sequences of moves by the proponent will make her win the game, and which sequences will make her lose the game.[1]

We do not need to use the dialogical conception when a deduction meets the logician's standard of presentation, spelling out every step in the argument and every premise. In such a case, there is simply a fact of the matter that a sequence of formulae which meets the highest possible standard has been written down. At least, there is such a fact of the matter given the prior choice of a logical system. And our concern here is with what may be done given such a prior choice. We are not concerned with results about logical systems that may be obtained by investigating the general properties of games and of possible sequences of moves, important though those results may be in other contexts.

The dialogical conception comes into its own when the logician's standard is not met to perfection, and that is when we need reassurance. The conception can reassure us if it leads us to see the process of writing deductions as one that builds in a search for flaws. We must however acknowledge that this does not give any guarantee that the search for flaws will be as aggressive as it could be. The reassurance we gain is therefore limited.

Mathematical practice provides examples of expectations and conduct that reflect the dialogical conception. When an important new result is announced, mathematicians want to enter into dialogue with the person or people who wrote

[1] Dutilh Novaes, "A Dialogical Account of Deductive Reasoning as a Case Study for how Culture Shapes Cognition", pages 461-463; Krabbe, "Arguments, Proofs, and Dialogues", sections 3.5 and 3.6.

the purported proof, so as to understand it and challenge any apparent weaknesses. They are not likely to assent to the new result until after this process has been completed. We can see an example of what happens when this process does not take place by looking at initial reactions to Shinichi Mochizuki's purported proof of the abc conjecture, announced in 2012. The proof was very long and complex, and it used novel concepts. Despite this, Mochizuki was initially reluctant to enter into dialogue about his purported proof, so the normal dialogical procedure was not followed.[2] This left mathematicians not quite sure what to make of his work. As at the time of writing, there is still no agreement as to whether the abc conjecture has been proved.

The focus of work on the dialogical conception has been largely on the proofs of mathematics, but the conception can be extended to arguments in the natural sciences, whether they are deductive or only come close to being deductive. The conception would however need some modification. In mathematics, the rules of deduction give a clear understanding of exactly when an opponent may object to a move. In the natural sciences, the rules of argument are not so completely formalized. There is therefore scope for it to be indeterminate whether a given objection would defeat a proponent.

Another control comes from insistence that everything that is needed to evaluate an argument should be set out for readers to consider. We shall give an illustration drawn from mathematics, and then note that the position in the natural sciences is not quite so good.

[2] Chen, "The Paradox of the Proof". A workshop was however held at Kyoto University in March 2015. Mochizuki also plans to participate by Skype in a workshop in Oxford in December 2015: Clay Mathematics Institute, *IUT Theory of Shinichi Mochizuki*.

The mathematical illustration is given by Kenny Easwaran, in his discussion of the reasons why mathematicians do not regard arguments which merely make it very probable that given results are correct as proofs of those results.[3] Easwaran argues that mathematicians are not generally content with proofs of results unless they are given all the material they would need to convince themselves that the results were correct. If a proof were merely probabilistic, they would typically need more information than was given, or even than could be given. They might for example need to know that the values on which the result was tested were selected at random. But even if the method of selection were set out and would manifestly yield a random selection, they would be unable to verify that the method had in fact generated the values used because if it were used again, it would generate different values. Readers would then have to take it on trust that the procedure had generated the values given. That trust might well be justified. Most mathematicians are perfectly honest, and most would not try to carry out some procedure, such as the selection of test values at random, without first ensuring that they had the skills to do so properly. But mathematicians are not inclined to rely on the testimony of others, even honest researchers, without seeing the underlying evidence. This is the important point here. The norm at work is that everything should be available for each reader of a paper to check. There is a separate norm in mathematics that conclusions should ideally be rendered certain rather than merely probable, but Easwaran's example is essentially about the need to present all the evidence, and only incidentally about conclusions' merely being probable.[4]

[3] Easwaran, "Probabilistic Proofs and Transferability".

[4] Work in mathematics that renders conclusions merely probable can however be useful. It can for example bring patterns to light or suggest approaches to proof: Borwein and Bailey, *Mathematics by Experiment: Plausible Reasoning in the 21st Century*, pages 2-3.

The norm of giving the readers of papers enough information that they need not take anything on trust is not applied quite so strictly in the natural sciences. It is standard in the natural sciences not to want to re-run every experiment that supports a result, and papers need not give enough information to make it feasible to re-run experiments in exactly the same form as was originally used. But reasonably detailed descriptions of work done are still expected, so that people who read papers can consider whether the work done might have been inadequate in any way.[5]

8.2.3 The role of the existing corpus

When the deductive route is used, the existing corpus of the relevant discipline is typically a vital source of premises. Arguments would be most unlikely to be deductive, or to approach being deductive, without premises taken from that source. We must consider whether our confidence should be affected by this reliance on the corpus.

Mathematics must be treated separately from other disciplines. It is built on the axioms and the definitions of identified entities that mathematicians have chosen. Theorems are secure in the sense that there is no point in questioning the ultimate starting points of their deduction, the axioms and definitions, because those starting points were chosen freely. (We here set to one side the concern that axioms might not be quite right for entities that were grasped independently, a concern that we noted in section 4.2.3.3, because it would always be possible to say

[5] For a discussion of differences between mathematics and other disciplines see Easwaran, "Probabilistic Proofs and Transferability", section 4.

that axioms which were not quite right for those entities were right for other, invented, entities.) The one exception is that it may be worth asking whether foundations are inconsistent, but that is hardly ever a serious worry. Apart from that risk, there is no scope for nervousness that researchers may be working against the background of the wrong corpus. They simply have the corpus that they have chosen by virtue of choosing axioms and definitions. There may be specific mistakes in mathematics that have so far gone unnoticed, but they are not to be corrected by paying more attention to the external world.

In disciplines other than mathematics, researchers also need to use the corpus in order to make progress. But they are exposed to the risk that elements drawn from the corpus may not accurately represent the world. To take an example from physics in which the risk has been made explicit, quantum electrodynamics and the standard model provide vital support for measurements of the fine-structure constant, but physics might move beyond those theories in ways that would undermine the measurements.[6] Such risks may be small, but they do exist.

We must therefore ask whether premises that are drawn from the corpus are adequately supported. If we cannot be confident that they are adequately supported, our confidence in claims that are reached by the deductive route must be diminished. Premises drawn from the corpus will be adequately supported, to the extent that the process by which they came to be accepted was itself adequately controlled. That is, we must look at the state of the relevant discipline as a whole.

[6] Hanneke, Fogwell and Gabrielse, “New Measurement of the Electron Magnetic Moment and the Fine Structure Constant”. The authors spell out their assumption that the standard model will not be superseded on page 120801-4.

8.3 The good explanation route

Very often, researchers find that while some claims may be deduced from evidence and the corpus, or may be reached from evidence and the corpus by reasoning that is close to deductive, those claims are not numerous enough to give a satisfactory account of a topic. Even if they are numerous enough, accounts of a topic which can be obtained in that way may not be the best accounts, for example the ones that have the greatest explanatory power. Researchers may therefore need to search more widely.

We shall now consider the route to accounts that involves a search for accounts which give good explanations, followed by the endorsement of accounts when the explanations found are good enough. We shall call this the good explanation route. The route encompasses abduction. And we shall use the word "explanation" for any explanatory account.

The search for good explanations is primarily important because researchers want to enlarge and improve the corpus of each discipline. But it has a special relevance in the context of our main question. If researchers search assiduously for good explanations, so that they do not just regard as good the first explanations they find but compare explanations and discard the comparatively poor ones, and if they are also strict in their appraisal of the quality of explanations, there is at least some reason to think that explanations which pass muster do reflect a correct understanding of the workings of the world. That should increase our confidence in the corresponding claims to explain and in claims that play explanatory roles.

We shall consider the notion of a good explanation, the use of the good explanation route to support claims within

explanantia, the process of search for good explanations, special issues that arise when researchers identify meanings that people studied attributed to items within their own experience, and controls over use of the good explanation route.

8.3.1 The notion of a good explanation

Those who seek to identify good explanations must have an idea of what makes an explanation a good one.

In the disciplines in which explanations take the form of theories that are applied on occasions when phenomena arise, that is, in the natural sciences and some of the social sciences, it is possible to draw on work that has been done to identify the qualities of a good theory. Identified virtues include those of empirical fit and adequacy, explanatory power, internal consistency and coherence, simplicity, consonance with wider theory, being the best available theory, fertility, the ability to unify different domains, and durability as the relevant discipline advances.[7]

We may extend the fruits of this kind of work to disciplines in which explanations do not routinely take the form of theories. We may speak of the qualities of explaining a great deal (or at least of being of a type of explanation that has wide application), of fitting very well with the existing corpus, and in some disciplines of fitting very well with an existing psycho-social understanding, where in both cases goodness of fit goes beyond mere consistency and extends to some degree of integration.

[7] This catalogue of virtues is taken from McMullin, "The Virtues of a Good Theory", pages 564-569.

In disciplines at all points on the scale, we may also regard the ability of an explanation to account for a wide variety of different and apparently independent pieces of evidence as a good sign. The phenomenon of a wide variety of pieces of evidence all pointing to the same conclusion is generally termed "consilience". One form of their pointing in the same direction is a single explanation's accounting for all of them: the factual correctness of the explanans is the conclusion to which they point, albeit not by entailing that conclusion. There is an argument made by Laura Snyder that the selection of an explanation on the basis of consilience differs from inference to the best explanation because the explanation originates in a different way. She argues that a relevant causal law emerges from the specific cases covered, rather than being identified separately and then applied to cases in order to bring them under a single law.[8] Snyder makes this point by reference to causal laws, but it is of more general application. It may also apply to distinguish the use of consilience from other forms of inference to good explanations generally, and not merely from inference to the best explanation. We need not however identify a separate route to explanatory accounts. Rather, we can see the search for consilience as one tool in the search for good explanations, and the occurrence of consilience as a source of confidence in the results of such searches.[9]

There is a related phenomenon that can give confidence and that is picked out and discussed in the social sciences. This is the phenomenon of successful triangulation. Triangulation involves the use of different methods, investigators, theories or sources to study the same phenomenon. If the same results are obtained in different ways, that can give

[8] Snyder, "Consilience, Confirmation, and Realism", page 134.

[9] An example that shows how consilience can be important is Forber and Griffith, "Historical Reconstruction: Gaining Epistemic Access to the Deep Past".

confidence in those results, whereas different results would cast doubt on some or all of the results.[10] Triangulation is not specific to searches for good explanations, but it is relevant to such searches. The results that are of particular interest in the context of such searches are those which show causal or other connections between phenomena. Since such connections are generally inferred rather than being manifest in the unanalysed evidence, successful triangulation, identifying the connections from several points of view, can be particularly reassuring. (As it happens, the role of triangulation in validating results is less in favour than it has been in the past.[11] But this is for reasons connected with theoretical conceptions of the social sciences. There is no reason why triangulation should not retain its practical role as a source of confidence.)

Triangulation can also play a role in the natural sciences, when the same phenomenon is studied by different groups of researchers or using different methods. Again, success comes when the same results are obtained. There is also scope to distinguish a variant that has been called multiple derivability, which involves approaching phenomena using methods that are based on different scientific backgrounds.[12]

We should not expect fully defined tests of goodness of explanation to be decisive in any discipline. Explanations that are good enough to get discussed are likely to pass the obvious tests that could be applied mechanically. There will then be a need for expert judgement to assess their relative merits.

[10] Rothbauer, "Triangulation".

[11] Moran-Ellis et al., "Triangulation and Integration: Processes, Claims and Implications", pages 47-49.

[12] Nederbragt, "Multiple Derivability and the Reliability and Stabilization of Theories", section 5.2.

8.3.2 Supporting claims

One use of the good explanation route is to support claims within explanantia. This use is commonly characterized as inference to the best explanation, also known as abduction, in its epistemological use to support claims rather than in its heuristic use to guide the search for explanations.[13]

The special feature of such reasoning is that there is inadequate support for claims within an explanans which is independent of the fact that the explanation is a good one. The quality of the explanation is taken to support at least the significant claims within the explanans. The demand for a single best explanation that is often made is a strict version of the condition we noted in section 5.1.2.2, that a claim can only derive support from playing an explanatory role or roles if the explanations of a given phenomenon in which it does so are better than explanations in which it does not do so. But it is also possible to apply some more relaxed version of the condition, for example a version which makes it enough to find a number of explanations that are on the whole better than explanations in which a claim to be supported plays no explanatory role. As we noted in section 5.1.2.2, scope to apply a relaxed version is more likely to arise when a claim to be supported is a claim of particular fact than when it is a general claim.

This source of pressure to pick out explanations that are especially good is absent when claims within explanantia are already established. And even in a discipline within which inference to the best explanation, or to several explanations that are better than others, is used to support claims within explanantia, it can still be acceptable to use other explanations of a given phenomenon for other

[13] For this distinction between uses see Iranzo, “Abduction and Inference to the Best Explanation”, section 2.

purposes. Having said that, a large number of explanations of a given phenomenon could give rise to concern. It would be likely that some of them would miss the point, or would be too weak to be worth considering. Thus at least some pressure to pick out explanations that are especially good can arise when the task is to support claims to explain. In disciplines low down the scale, where there is a desire to find single explanations, pressure is especially likely to arise in this way.

8.3.3 The search for good explanations

Searches for good explanations need to proceed appropriately. We shall now consider the need to search widely, the need for searches to be guided and the possible recharacterization of evidence, before commenting briefly on inductive argument.

8.3.3.1 The need to search widely

The search for explanations must be wide enough to bring an appropriate range of candidate explanations into consideration. The candidates all need to be considered, even if some of them only merit very brief consideration and can be discarded quickly. There are several reasons why this is so.

The first and most obvious reason is that if researchers do not search widely, or do not consider some of the candidates they find, they may fail to endorse some explanations, and thereby fail to accept some claims to explain and claims within explanantia, when they could have done so. Opportunities to expand the corpus would then be missed.

Another reason, which relates more directly to our concern with confidence, is that if candidates are not found or are excluded from consideration, some of the candidates that are considered may appear to be especially good when in fact they are no better than some of those that are not found or are excluded. It is true that goodness and exceptional goodness are different things. An explanation may be good, whether or not it is exceptionally good. To that extent a failure to consider other explanations might not seem to matter, especially when it is acceptable and perhaps even beneficial to make use of several explanations at once, for example in order to give a full picture. But the mere fact that an explanation is distinguished among those considered will encourage researchers to endorse it. A failure to consider explanations that should have been considered, whether because not enough effort was made to find them or because they were rejected out of hand, may therefore cast doubt on some decisions to endorse other explanations.

A third reason, which is closely related to the second one, is this. Even in a discipline in which several explanations of the same phenomenon are welcome, not just any explanation is good enough to support its claim to explain. Some candidates may be so much better than others that the worse candidates should be regarded as missing the point. Then researchers should not assent to the claims to explain of the worse candidates, because those candidates could not be seen as representing the world appropriately. The degree of support for claims that played explanatory roles within the worse candidates would in turn be put in question. A wide search is needed because researchers can only see that candidates miss the point if they also have in front of them candidates that hit the point.

Finally, when the task is to support a claim within an explanans, a wide search is essential because if a search

has not been wide, the fact that candidates in which the claim plays explanatory roles are better than those which have been found and in which it does not do so will not carry much weight if there might be some undetected good explanations in which the claim did not play any explanatory roles.

This last risk presented by a failure to search widely might appear to be insuperable. A wide search might reduce the risk of wrongly identifying some explanations as good enough relative to all possible explanations, but however widely researchers searched, there might always be some other explanations of high quality they had missed.

This problem of unconceived alternatives has been discussed extensively, although much of the discussion has concerned the prospects for scientific realism rather than the confidence we might have in accepted claims.[14] There are however two reasons why we should not be greatly troubled, whichever side of the philosophical argument about realism we may favour.

The first reason, which is only relevant on occasion, is that it is possible for evidence itself to count against the existence of a wide range of alternatives. Some evidence, particularly evidence of the repeated predictive success of a theory, may be such that it would be most unlikely to arise if there were many alternatives.[15]

[14] Stanford, *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives* (Stanford sets out the problem in chapter 2); Chakravartty, "What You Don't Know Can't Hurt You: Realism and the Unconceived"; Egg, "Expanding Our Grasp: Causal Knowledge and the Problem of Unconceived Alternatives".

[15] Dawid, Hartmann and Sprenger, "The No Alternatives Argument".

The second reason, which is of more general application, is that we resolved in section 1.1.5.3 not to concern ourselves with unspecified and large-scale risks. At least some instances of the problem of unconceived alternatives, and most of the instances that might have a dramatic impact on the status of accepted claims, can be regarded as examples of the unspecified and large-scale risks that may be disregarded when our concern is with the confidence we may sensibly have for the time being.

We shall return to the problem of unconceived alternatives, and note an additional source of reassurance, in section 8.3.5.3.

8.3.3.2 The need for searches to be guided

Although searches for candidate explanations must range widely, it would be very inefficient to manufacture candidates in an unconstrained way. A great deal of effort would be expended, both in manufacturing them and in appraising them, only to find that most of them were not much good. It is therefore important to the efficient conduct of a discipline that searches should be guided. Both the existing corpus and paradigms are important.

The existing corpus

The existing corpus can limit the options for explanation. An explanation that is inconsistent with claims in the corpus is ruled out unless the evidence is strong enough to persuade researchers to renounce or revise the relevant parts of the corpus. But in disciplines low down the scale, in which there are many relationships between propositions that are either deductive or almost as strong, the corpus does

more than exclude certain explanations. An explanation is expected to make connections with the corpus, where those connections are expressed in strong relationships between propositions. Existing concepts used in the discipline and existing laws need to be put to work. The result can be a substantial narrowing of the options. That narrowing makes the task of appraising candidates easier than it would otherwise be, although at the small risk that good explanations may be overlooked simply because of their lack of integration with the corpus.

In disciplines higher up the scale, where strong relationships between propositions are less common, the corpus cannot play this direct role in limiting options to the same extent. Researchers can make up for the reduced extent to which it can play this role by searching for explanations that make many connections of a looser nature with elements in the corpus.

Whether enough connections are made is however a matter of judgement. It might be feared that there would be a natural inclination to exercise judgement in ways that would lead researchers to concentrate on candidates that were especially easy to relate to the corpus. Then researchers might not give due weight to hints in the evidence that new and unfamiliar lines of thought would be appropriate. We must acknowledge the risk, but we should not underestimate the capacity of researchers to avoid the trap. While their starting points can strongly influence what they find, the evidence can lead them to consider claims that they had not even had in mind as possibilities when they started work.[16]

[16] Wylie, *Thinking from Things: Essays in the Philosophy of Archaeology*, page xiv. Wylie writes without specific reference to the stage of searching for explanations, but the point does apply to that stage.

It is not only large-scale theories or principles that can be significant. Detailed results can also limit the options for explanation. When a discipline reaches a stage in its development at which there is a recognized need for relatively large-scale explanatory accounts, detailed experimental results can play precisely this limiting role.[17]

We shall now turn from the corpus to paradigms. We shall consider the notion of a paradigm, the role of paradigms in guiding searches for explanations, and the problem of competing paradigms.

The notion of a paradigm

A paradigm is a way to approach a field of study that leads researchers to favour certain ways to organize the corpus, certain overall pictures of the world (pictures which may or may not amount to theories), certain ways to work, and certain types of finished account.[18] For example, two contrasting paradigms are the integrative approach of systems biology and the reductionist approach of molecular biology.[19] Another example of a paradigm is the view that social change is to be explained primarily in economic terms. It is a matter of judgement when a general approach

[17] The evolution of human cognition is a current example. See the papers in Heyes and Frith (eds.), "New Thinking: The Evolution of Human Cognition".

[18] A comparable notion of paradigms is given in Pocock, "The Reconstruction of Discourse: Towards the Historiography of Political Thought", page 72 – although Pocock has some concerns about the notion in the context of his own field of work. For Thomas Kuhn's view of the roles and the effects of paradigms in the natural sciences see Kuhn, *The Structure of Scientific Revolutions*. Our notion of a paradigm is broader than Kuhn's, so we need not hesitate to apply it across all disciplines.

[19] For the contrast between systems biology and molecular biology see Noble, *The Music of Life: Biology Beyond the Genome*, pages x-xi.

is sufficiently well-defined and stable to be regarded as a paradigm. One might for example regard the emphasis of Fernand Braudel on the longue durée rather than on individual historical events as a paradigm, but think that the tradition of the Annales school of historians more generally was too ill-defined and too mutable over the history of the school to count as a specific paradigm.[20]

There is a hazy boundary between examples of two paradigms leading to different approaches to the same topics, and examples of different approaches leading to the study of different topics. We can take three examples from the study of history. The first example is that of treating cultural dispositions and constructs either as arising out of social structures, or as having independent lives and efficacy.[21] This is an example of two paradigms for treatment of the same topics. The second example is that of historians discussing nineteenth-century British political thought either in terms of the history of political theory, or in terms of the history of social science.[22] This would probably, but not indisputably, represent two paradigms for treatment of the same topic. The third example is that of choosing to write a political history or a social history of a given country or period. That would be a choice between two topics. By contrast, a choice of whether to seek political or social explanations of some phenomenon that was already specified in detail would be a choice between two paradigms for treatment of the same topic.

[20] For Braudel see Clark, "The *Annales* Historians". For the school as a whole see Burguière, *L'École des Annales. Une histoire intellectuelle*.

[21] Cabrera, *Postsocial History: An Introduction*, chapter 1, section 1.

[22] Collini, Winch and Burrow, *That Noble Science of Politics: A Study in Nineteenth-Century Intellectual History*, pages 7-12. The two approaches are singled out on pages 9 and 10 respectively. The authors do not wholeheartedly adopt either approach.

Not only is the boundary between a choice of topics and a choice of paradigms for tackling the same topic a hazy one. It is also not always clear when a paradigm has developed while remaining the same paradigm, and when it has been replaced by a new paradigm. Both of these difficulties may be illustrated by the rise of a new type of social history, the history of a society or Gesellschaftsgeschichte, in the 1960s and 1970s, and criticism of its first form that resulted in the rise of the history of everyday life or Alltagsgeschichte in the 1980s.[23] Does the move from a concentration on social structures to a concentration on the small-scale and the everyday represent a change of topic or a change of paradigm, and does Alltagsgeschichte represent a new paradigm or a development of the existing paradigm of Gesellschaftsgeschichte? Fortunately, we may simply note such niceties.

There is also no sharp demarcation of the contents of paradigms from the contents of corpora. Claims within a corpus may be general enough, or sufficiently taken for granted by researchers, that they should also be regarded as elements in paradigms. Spyridon Orestis Palermos has suggested that scientific theories can function rather like language. They can extend the cognitive faculties of researchers, while being so well-integrated into the ways in which researchers think that they do not even notice they are using them.[24] We could regard theories like that as having become parts of paradigms, even while they retained

[23] Kocka, *Industrial Culture and Bourgeois Society: Business, Labor, and Bureaucracy in Modern Germany*, pages 275-279. For more examples of the many types of history that may be written, the choice of at least some of which would amount to the choice of different paradigms for treatment of the same topics, see Burke (ed.), *New Perspectives on Historical Writing*; Kramer and Maza (eds.), *A Companion to Western Historical Thought*.

[24] Palermos, *Extending Cognition in Epistemology: Towards an Individualistic Social Epistemology*, section 3.4.2.2.

their status as sets of claims within their disciplines' corpora.

We shall not extend the notion of a paradigm to bring within its scope the personal biases of researchers. We should nonetheless be alert to the risk of such biases. On the bright side, there is reason to think that the damage done by personal bias can be reduced if researchers adhere to appropriate standards and take appropriate care.[25] And there is always the possibility that damaging biases will be criticized by other researchers.[26]

Paradigms as guides in searches

A paradigm may guide the search for candidate explanations. It helps if researchers come up with candidates in an intelligent way, so as to identify a wide enough range of candidates while not wasting time on candidates that have little or no hope of being good enough to be endorsed. A paradigm can perform this role very straightforwardly. It can shape the intellects and the imaginations of researchers.

A paradigm can give more specific guidance by steering researchers toward explanations of particular types. For example, there may be a distinct preference within a discipline for explanations that identify mechanisms, and

[25] McCullagh, "Bias in Historical Description, Interpretation, and Explanation", section 4.

[26] A classic example of ferocious criticism of alleged personal bias is Hexter, "The Burden of Proof: Christopher Hill, *Change and Continuity in Seventeenth Century England*". J. H. Hexter accused Christopher Hill of reading sources in order to support a view he had already selected, and noted that Hill could achieve his end because when there is a large enough set of sources, it is possible to find sources to support any given view. Hexter's attack has however been criticized in turn: Palmer, "The Burden of Proof: J. H. Hexter and Christopher Hill".

that do so in ways which respect certain constraints. That preference will be incorporated in a paradigm.[27] The presence of such influences may very well increase our confidence in the results of work, because it will suggest that researchers have worked under pressure to meet standards that are imposed by the whole community of researchers.

Competing paradigms

There are sometimes several different paradigms in play in a discipline. Academic conflicts may result. The proponents of some explanations may not see any merit in other explanations, because their proponents approach the relevant topics in different ways.[28] How should our confidence be affected?

There will sometimes be little adverse effect. The various paradigms may all be reasonably well-established and reputable, and there may be no direct conflicts between them. Then we can take any given paradigm to be appropriate, and pose our main question about influences on our confidence against the background of our having done so. We discussed this option in section 1.1.4.1.

This option is not always available. There may be something in each paradigm which implies that the use of other paradigms would lead researchers to make incorrect claims, rather than merely to make claims that were not worth making. If in addition there is no decisive majority in the

[27] Lindley Darden has explored this kind of influence in connection with biology: Darden, *Reasoning in Biological Discoveries: Essays on Mechanisms, Interfield Relations, and Anomaly Resolution*, section 2.6.

[28] For examples of different approaches in anthropology see Kloos, "Multiple Images of Ethnic Reality: Beyond Disagreement?", pages 77-84.

research community in favour of one paradigm, we may well doubt that any paradigm has enough credibility to give confidence that its use is a good route to correct claims. In section 6.3, we considered the advantages of pluralism. Here we note that a plurality of paradigms can be unnerving. We have here a wider version of the concerns about the implications for our confidence of choices of formative concepts that we discussed in sections 6.2.3.2 and 6.2.4.3.

8.3.3.3 The recharacterization of evidence

One possible move in the search for explanations is to recharacterize evidence. Different characterizations may allow different explanations to be given. Researchers can try characterizations, see what explanations they make possible, judge whether the explanations are good ones, see whether the characterizations are well-supported by the overall context provided by all the available evidence, the corpus of the discipline and any relevant psycho-social understanding, and continue to make adjustments to characterizations, explanations and their view of the overall context until they reach the best available balance. But the scope to recharacterize evidence varies considerably from one discipline to another.

In disciplines that are low down the scale, such as physics and chemistry, it is usually agreed which concepts must be used in order to characterize evidence fully, so there is usually little or no scope to add to what all or most researchers agree must be said about evidence. This means that there is usually little or no scope to change the characterization of evidence itself. (A choice of explanations that may be built on a given body of evidence is however not excluded.)

In higher disciplines, there is unlikely to be agreement on a complete list of concepts that should be used to characterize evidence fully, even though some concepts may be agreed to be ones that must play roles in characterizing evidence. This means that there is scope for different researchers to say different things about the same evidence. To take an example from archaeology, archaeologists may (but need not) interpret remains semiotically, and they may do so in various ways.[29]

There are however limits to the scope to recharacterize evidence, even in disciplines that are high up the scale. The relevant corpus, the norms of the discipline and any relevant psycho-social understanding must all be respected, or a good case must be made for changes to them.

8.3.3.4 The place of induction

A standard category of argument is that of induction in the narrow sense of arguing for a proposition by noting supportive results from a large number of cases while there are no cases, or acceptably few cases, on the other side. We shall not treat this as a separate route to accounts. Induction may draw a regularity to the attention of researchers. Then they may seek an explanation. But that search for an explanation will normally amount to use of the good explanation route. Observed regularities may then offer support for the factual correctness of an explanans by being regularities that the explanans would explain, but by that stage, researchers would already have trodden the route to the explanans. Moreover, work that has been done on the nature of the inductive reasoning process, including

[29] Vianello, "Can Archaeology's 'Ritualistic and Symbolic Artefacts' Be Interpreted Semiotically?"

work on how enumeration can be supplemented by more sophisticated techniques, does not prevent us from taking the view that neither induction nor any comparable process should be treated as a separate route to accounts.[30]

8.3.4 Attributed meanings

We shall now consider explanations of a special type, those which identify meanings that people who are studied attributed to items within their own experience. The items in question may be events, objects, social structures, social roles, or human character traits or actions. For convenience we shall speak as if the people studied lived in the past, but the same approach could equally well be used when explaining life in contemporary cultures. And we shall stipulate that the identification of attributed meanings requires not only their distinction from obvious alternative meanings, but also their specification with reasonable precision.

Explanations of this type confer Verstehen in the sense that we set out in section 5.6.1. Verstehen in our sense is conferred when the reader of an explanation obtains an understanding of why things happened in the way they did by seeing the people studied as having possessed human points of view, just as she possesses one, and as having been motivated by considerations of the same general type as those that would motivate her. The reader could then articulate her understanding to another human being by setting out people's circumstances, characters, desires, worries and so on, and explaining their conduct in those terms.

[30] One example of such work is Harman and Kulkarni, *Reliable Reasoning: Induction and Statistical Learning Theory*.

The particular route to Verstehen that concerns us here achieves the desired result through the identification by researchers of meanings that the people studied attributed to events, objects, social structures, social roles, and human character traits and actions. We are concerned specifically with meanings that researchers, being human, can directly appreciate would have made the events, objects and so on significant. Given that significance, and the fact that the people studied would be seen as having possessed human points of view, it would be immediately clear how the presence of the events, objects and so on would have influenced the forms of life or the specific conduct of those people. Most non-human rational beings, on the other hand, would not directly appreciate how the presence of the events, objects and so on could be expected to influence forms of life or specific conduct, because they would not see directly the impact of the relevant attributions of meaning.

For example, human readers can make sense of a choice of white horses to draw a Roman triumphal chariot, once it is pointed out that Romans would attribute a specific meaning to such fine creatures, that of an association with divinity.[31] Human readers, appreciating as they do the significance of divinity even if they have no belief in the divine, would require no further explanation of a choice of white horses. There would be much more to be said about the nature of Roman ideas of divinity and about the psychological, social and political implications of the association of a person with the divine, but human readers would grasp the main point, the reason for a choice of white horses, without further ado. Most non-human rational beings, on the other hand, would need to have the significance of divinity explained to them. They would be in the position of Charles Taylor's Alpha

[31] Beard, *The Roman Triumph*, pages 233-236. While the tradition is clear from surviving sources, the record is unfortunately not solid on what happened on specific occasions.

Centaurans.[32]

In order to confer Verstehen, attributed meanings must be ones that resonate with current readers. Sometimes there is a strong connection between past meanings and currently available meanings, simply by virtue of the fact that differences of direct relevance are not great. To return to the example of Romans creating associations with the divine, modern ideas of the divine differ from Roman ideas but they are alike in the relevant way: it is clear that one should be in awe of the divine. But sometimes there is no such straightforward connection, or only a weak one. There may be some commonality between past meanings and currently available meanings, but also some significant differences that are directly relevant. And sometimes, the disappearance of a way of life can make attributed meanings inaccessible.[33] In such difficult cases, it may be necessary to engage in an act of imagination, thinking oneself into the frame of mind of people who were culturally different, or at least into a frame of mind that is reasonably close to their frame of mind. This is a matter of degree. When the distance becomes too great, modern readers may find comprehension almost as difficult as some non-human rational beings who were not very different from human beings might find it. And it is a delicate matter to capture the notion of a change in frame of mind precisely. It does not have to be seen as a change in a single frame of mind. It can also be seen as the acquisition of a second frame of mind without abandoning the first one.[34] But given that connections between past meanings and currently available meanings sometimes exist, and that it is possible for readers to adapt their frames of mind or to adopt additional ones, we shall simply speak of the attribution of meanings by

[32] Taylor, "Self-Interpreting Animals", sections 1 and 2.1.
[33] Lear, "What is a Crisis of Intelligibility?"
[34] Elliott, *History in the Making*, pages 32-33.

the people studied, and shall take it that the identification of those attributed meanings allows readers to achieve Verstehen because the meanings can resonate with them.

We single out the approach of identifying attributed meanings because the process of identification has its own complexities, and those complexities may lead us to have special concerns about claims that have come to be accepted following use of the approach. We consider this approach under the general heading of the good explanation route because it is a form of non-deductive search for explanations that may be satisfactory to varying degrees.

8.3.4.1 Scope to identify meanings

The identification of meanings that people are taken to have attributed requires careful consideration of both the detailed evidence and the overall pictures that emerge of the psyches and societies of the people studied.

Attributed meanings cannot be identified at will. Identifications must not be at variance with claims in the corpus unless a good enough case can be made to change the corpus. Identifications must also be consonant with some psycho-social understanding that can be attributed to the people studied. More generally, identifications are constrained by the linguistic and historical contexts of those people, and by what is surmised about their psychologies. Disciplines also have norms that govern identifications, requiring certain levels of support from evidence, although those norms are likely to be implied by practice rather than being explicit. Finally, general principles may be brought to bear in the evaluation of work that has involved the identification of attributed meanings. For example, the evaluators of work may examine whether some identification of

supposedly attributed overall meaning is consistent with the corresponding identifications of supposedly attributed meanings of parts of the whole.

We shall now turn to the hermeneutic tradition, which can be a source of controls over the identification of meanings.

8.3.4.2 The hermeneutic tradition

The most obvious items to which meanings have been attributed are texts. It can be a tricky business to identify meanings that were attributed in the past.

The interpretation of texts has been the traditional task of hermeneutics. One focus of work within the hermeneutic tradition has been the problem of how to establish meanings that current researchers should attribute to texts. But hermeneutic work also involves establishing what the authors of texts meant and what readers at the time of writing took them to mean. There will at that stage be an exercise in the identification by current researchers of meanings attributed at the time of writing. The hermeneutic tradition can therefore be a source of controls over the current identification of meanings that were attributed to items in the past, both texts and other items.

We shall borrow selectively from the tradition. The approach of the part of the tradition that is relevant to our concerns is to make sense of a text both by identifying meanings of its parts, the words and sentences that have meanings given by the rules and the usage of the relevant language, and by identifying meanings of the whole text. This part of the tradition is associated with Herder, Schleiermacher, Droysen and Dilthey.[35] Details

[35] For an account of the development of the relevant kind of

varied considerably as between these authors, and further variations were introduced later. We shall not however set out to be faithful to the detail of any particular variant of the tradition. Such fidelity is not important in relation to our main question.

Work on a text is guided by awareness of a context. The context includes the author's entire body of writing, the ways in which the relevant language was used by others at the time and, in some versions of the tradition, the author's actions and non-linguistic historical context. Researchers can go back and forth between parts of a text and the text as a whole, repeatedly making adjustments. The identification of a meaning of the text as a whole at the time of writing is constrained both by reference to the latest identifications of meanings of the parts and by reference to the wider context, especially those elements that would indicate the author's psychological traits. These constraints would allow some meanings of the whole to be identified while ruling out other meanings. Identifications of meanings of the parts at the time of writing are likewise constrained both by reference to the latest identified meaning of the whole and by reference to the wider context, especially the state of the language at the time. These constraints allow some meanings of the parts to be identified while ruling out other meanings. Moreover, it is not only meanings of the whole and of parts that can be adjusted. Researchers use background theories, principles and views of the context when identifying meanings, and there may be scope to make

hermeneutics in the hands of Herder see Michael Forster's introduction to Herder, *Philosophical Writings*, pages xiv-xxi. For Schleiermacher see Hausheer, "Three Major Originators of the Concept of *Verstehen*: Vico, Herder, Schleiermacher", section 4. For Droysen see Maclean, "Johann Gustav Droysen and the Development of Historical Hermeneutics". For Dilthey see Bulhof, *Wilhelm Dilthey: A Hermeneutic Approach to the Study of History and Culture*, chapter 4; Rickman, *Wilhelm Dilthey: Pioneer of the Human Studies*, chapter 10.

adjustments to any of these. The possibility of adjusting theories becomes more conspicuous when we transfer our attention from texts, work on which need not require much theoretical background (although it may do so), to the task of making sense of non-textual remains, a task in which theoretical background is more likely to be required.[36]

How researchers start their work, and what moves they make in that work, will of course be influenced by their own natures and intellectual contexts. Different theoreticians of hermeneutics may take different views of the form and extent of such influences. They therefore take different views of the severity of the consequences of that influence for hopes of working methodically to identify appropriate meanings. But there is reason to be optimistic. Even if we were to put the problem in the terms that Hans-Georg Gadamer developed out of Martin Heidegger's work, and were to see researchers as always projecting meanings that reflected expectations, there would still be ample scope to discriminate between good and bad identifications of meanings.[37]

The influence of the natures and contexts of researchers does however give rise to a complication that affects the appraisal of hermeneutic work. Any view of the process

[36] Compare the remarks in Kosso, *Knowing the Past: Philosophical Issues of History and Archaeology*, pages 67-68, on the hermeneutic interaction between middle-range theories and evidence.

[37] For the view that Gadamer developed see Gadamer, *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik*, Band 1, Teil 2, 2.1.a.a ("Heideggers Aufdeckung der Vorstruktur des Verstehens"), translated as Gadamer, *Truth and Method*, part 2, 2.1.a.1 ("Heidegger's Disclosure of the Fore-Structure of Understanding"). Gadamer's concerns differed from those of earlier thinkers. He was more interested in the nature of the encounter between reader and text than in the technical task of identifying appropriate meanings. Nonetheless, his formulation of the process is relevant to the question of how easy it may be to identify appropriate meanings.

that gives a prominent role to the ways in which researchers think, whether in the way that Gadamer did or in any other way, opens the door to a new type of adjustment, over and above adjustment between the identifications of meanings of parts and of the whole. This new type is adjustment of the ways in which researchers think so as to bring those ways into closer alignment with the ways in which the people studied thought. This is a version of the adjustment of one's own frame of mind to or toward the frame of mind of the people studied that we mentioned in section 8.3.4. Gadamer brought such adjustments under the rubric of Horizontverschmelzung, the fusion of horizons (the horizons of the researcher and of the people studied).[38] Adjustment of this type is not only a way to allow attributed meanings to resonate with modern readers. It can also play a valuable role in making it possible to arrive at an appropriate identification of attributed meanings. Researchers might however accidentally adopt a way of thought that was not in fact close to the ways in which the people studied thought, but that made it easy to argue for an identification of meanings that was plausible because everything seemed to fit together well. Controls over the process of identification are therefore vital.

Fortunately, controls are available. The hermeneutic tradition can be exploited as a source of controls over the identification of meanings that were in the past attributed to items generally, and not just to texts. We have already noted that identifications are constrained by linguistic and historical contexts, and by surmises about the psychologies of the people studied. Such contexts and surmises matter in relation to items generally, in much the same way that

[38] Gadamer, *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik*, Band 1, Teil 2, 2.1.d ("Das Prinzip der Wirkungsgeschichte"), translated as Gadamer, *Truth and Method*, part 2, 2.1.b.4 ("The Principle of History of Effect (Wirkungsgeschichte)").

they matter in relation to texts. The tradition also supplies a model for thinking systematically about different types and levels of work, about objectivity, and about circularity in reasoning.[39]

8.3.4.3 Items other than texts

We shall now give two examples of the identification of meanings attributed to items other than texts. One relates to physical objects, and the other to a social role.

It has been argued that identities, in the sense that could be associated with magical powers, may have been attributed to late bronze age and early iron age weapons, changing the social significance of the relevant weapons and explaining any abnormally lengthy preservation of some weapons (although as it happens, evidence of lengthy preservation is not generally available).[40] The argument rests not only on comparable attributions of identity in myths, which indicate elements in the overall psyches of those who made and used the weapons, but also on a careful examination of marks on weapons that have survived. A context of thought and detailed features of artefacts are therefore brought together to support the identification of attributed meanings.

An understanding of how, in the high middle ages, knightly status was heavily invested with the meaning of prowess in combat can play a role in painting a comprehensible picture of patterns of violence at the time, and in making sense of the reactions of contemporaries. As with the previous example, it cannot be assumed that the status was invested with this meaning. The evidence must be studied closely. A study of the details of texts that set out the deeds of both

[39] Seebohm, *Hermeneutics. Method and Methodology*, chapter 6.
[40] Pearce, "The Spirit of the Sword and Spear".

real and fictional characters can show that the status was invested with the meaning. A study of the context provided by other beliefs and by the institutions of church and state shows how this could have happened, and also helps to make sense of the detailed evidence.[41]

In these examples we see interplay between information about details and descriptions of the context that is given by the psyches and the societies of the people studied. We do not see quite the same pattern as we may see when hermeneuticists work on texts. This is because when we move away from texts, we move away from parts and wholes having meanings of the same general kind. There may indeed be nothing that corresponds to a whole text made up of meaningful parts. Instead there may be only one or more events, objects, social structures, social roles, character traits or actions to which some meanings are thought to have been attributed, items which resemble parts of a text in that they are not themselves made up of meaningful parts, and the context that constrains identifications of attributed meanings. The process is then one of reaching reflective equilibrium between identifications of attributed meanings and descriptions of the context. But although the picture may have fewer strata than would be normal in work on texts, the forms of control over identifications of attributed meanings that the hermeneutic tradition offers can still do useful work. (An alternative would be to regard the context as corresponding to a whole text, but again the parts and the whole would not have meanings of the same kind, and there would still be fewer strata than when there were parts of a text, a whole text and a context.)

[41] Kaeuper, *Chivalry and Violence in Medieval Europe*. Chapter 7 sets out the association with prowess, and Kaeuper makes the case that there was a strong association in real life as well as in literature from page 139 onward. Chapters 3 to 6 set out the context.

There is one condition we have already mentioned, and to which we now return in order to note that it is not a very demanding condition. We can turn to the hermeneutic tradition to the extent that it sets itself the task of identifying what the authors of texts meant and what readers at the time of writing took them to mean, that is, to the extent that it sets itself the task of identifying meanings that were in the past attributed to texts. There are other hermeneutic tasks, in particular the task of working out what a text should mean to the modern reader, and working out what meaning a text might have in isolation from authorial intention or the ways in which readers at the time of writing might have taken it. Since our interest is in researchers who identify the meanings that human beings attributed to events, objects, and so on, the tasks of the tradition need to include the task of working out what texts meant to their authors and to readers when they were written. Fortunately that task is included, at least in a form that suffices for our purposes. We need not make the controversial claim that authorial intention determines anything that should be regarded as the one true meaning of a text.[42] Nor need we make the implausible claim that researchers could routinely determine authors' intentions precisely and beyond argument.

The condition does however mean that we should not draw on every type of hermeneutic work. For example, the identification of latent Sinnstrukturen and objective Bedeutungsstrukturen that is the task of Ulrich Oevermann's objective hermeneutics differs from the type of work that interests us here.[43]

[42] Two papers that outline some of the issues and that may serve as introductions to the controversy are García Landa, "Authorial Intention in Literary Hermeneutics: On Two American Theories"; Weberman, "Gadamer's Hermeneutics, Non-Intentionalism and the Underdeterminedness of Aesthetic Properties".

[43] Oevermann, "Manifest der objektiv hermeneutischen Sozi-

8.3.4.4 The risk of incorrect specification

We shall now turn to what might go wrong when attributed meanings are identified. We shall be particularly concerned with the risk that meanings which may be identified correctly to the extent of distinguishing them from obvious alternatives may nonetheless be specified incorrectly. We shall mention specification separately from identification to emphasize this, even though we have stipulated that identification requires specification with reasonable precision.

The meanings that modern researchers seek to identify and specify will typically have been attributed through processes that rendered the meanings less than perfectly stable. The people concerned will have attributed meanings in a whole complex pattern that may have evolved as time passed, with changes to some attributions of meaning affecting other attributions. And the attitudes of the people concerned may also have changed. Moreover, the specification of meanings is far from being governed by a well-defined algorithm, even in the highly-developed and reflective discipline of anthropology. Some of the difficulties that arise when seeking to make sense of other societies have been set out, for example in the work of Charles Taylor.[44] But even if the difficulties are well-understood, they have not been dissolved. We may be concerned that the difficulties might lead to incorrect specification.

There is good reason to be concerned. Differences between the conceptual schemes of researchers and those of the

alforschung", item 1 ("Latente Sinnstrukturen und objective Bedeutungsstrukturen statt subjektiver Dispositionen"). The terms could be translated as "latent sense-structures" and "objective meaning-structures".

[44] Taylor, "Interpretation and the Sciences of Man"; Taylor, "Understanding and Ethnocentricity".

people being studied can make it easy to specify meanings wrongly, because concepts used by researchers may be inappropriate. The difficulty of correcting for conceptual differences is evidenced by the fact that anthropologists need to take great care when identifying and giving content to concepts that would be appropriate to an emic approach, an approach that sought to reflect the ways of thought of the people studied.[45] There is also a risk that the language of researchers may not have the words to indicate accurately the contents of some concepts, contents that were perfectly clear to the people studied, and that researchers may therefore either misrepresent the contents or conclude, wrongly, that they were more or less indeterminate.[46] Such deficiencies of language may leave researchers unable to specify attributed meanings accurately. A further risk arises in connection with concepts that the people studied used to attribute meaning and value but that suffered from incoherence. Researchers may deny the incoherence in the interests of doing supposed justice to the people studied, and may thereby misunderstand both the concepts and the culture.[47]

[45] For the emic approach, and the contrasting etic approach that seeks to identify and give content to cross-cultural concepts, see Barnard, "Emic and Etic"; Berry, Poortinga, Breugelmans, Chasiotis and Sam, *Cross-Cultural Psychology: Research and Applications*, pages 23-24 (of the third edition; pages 291-292 in the second edition). The latter text notes the benefit of using a careful iterative approach. For difficulties in making use of concepts that would be appropriate to an etic approach and in grasping concepts that would be appropriate to an emic approach see Helfrich, "Beyond the Dilemma of Cross-Cultural Psychology: Resolving the Tension between Etic and Emic Approaches", pages 133-138.

[46] For an example in relation to the interpretation of texts see the discussion of the concept of virtù as used by Machiavelli and his contemporaries in Skinner, *Visions of Politics: Volume 1, Regarding Method*, pages 48-49.

[47] Gellner, "Concepts and Society", sections 15 to 17.

Such concerns should not drive us to regard the task of identifying and specifying attributed meanings as impossible. There may always be a risk of mistakes, but the incidence and the magnitude of errors may be reduced to tolerable levels. In particular, a careful study of the history of concepts can expose areas of risk and can help to bridge the gap between past and present.[48] We may have a conception of reduction to tolerable levels even if we do not believe that there is any objectively perfect specification of attributed meanings by reference to which errors could be measured. One can get close enough to a hazy target. To put optimism in another (and not equivalent) way, it is possible to say something determinate even if there is no prospect of an end to debate over attributed meanings.[49] Moreover, since our concern is only that of whether researchers can identify and specify attributed meanings with reasonable success, we need not engage in debates as to appropriate forms of hermeneutics, debates such as those in which Hans-Georg Gadamer, Jürgen Habermas and Paul Ricœur have engaged.[50] We only borrow from the hermeneutic tradition. We do not embrace the whole of it, or even the whole of one strand within it.

Given the reasons to be concerned, our confidence in claims that have been reached through the identification of attributed meanings is likely to be heavily influenced by our view of the efficacy of controls over such work. Is enough attention paid to the need for consistency between meanings supposedly attributed to wholes and parts or to

[48] Koselleck, "*Begriffsgeschichte* and Social History", especially the final section, "On the Theory of *Begriffsgeschichte* and of Social History".

[49] This point is made in relation to authors' intended meanings of texts in Skinner, *Visions of Politics: Volume 1, Regarding Method*, page 124.

[50] For an introduction to those debates see Schmidt, *Understanding Hermeneutics*, chapter 7.

different items? Is a careful enough study of the history of concepts made? What other controls are there? Do all of the controls together reduce the risk of mistakes to a tolerably low level? There is no general rule that will answer such questions. We must exercise judgement when we consider how much confidence to have.

Finally, such concerns do not extend to all disciplines. The hermeneutic approach to interpretation has no place in most of the natural sciences, so our concerns do not extend to them. It is perfectly possible to make a case for the role of interpretation in the natural sciences, but that requires a notion of interpretation which turns out to allow the natural sciences to keep their distance from work of a hermeneutic nature, even though there might be roles for procedures that were analogous to those of hermeneutics.[51]

8.3.5 Controls

Controls matter in relation to all ways of using the good explanation route, not merely those that involve the identification of attributed meanings. We shall now examine controls, the application of which may support our confidence both in claims to explain and in claims that play explanatory roles. We shall consider scrutiny, decisions that researchers must take, controls that relate specifically to arguments that some explanations are better than others, and contributions that formal epistemology might make.

[51] Such a case for the role of interpretation is made in Faye, “Interpretation in the Natural Sciences”.

8.3.5.1 Scrutiny

Our confidence in claims that come to be accepted following use of the good explanation route depends on the satisfaction of several conditions, some of which emerge from our earlier remarks. The corpus and paradigms must limit the options appropriately, but they must not lead researchers to exclude candidate explanations for inappropriate reasons. Researchers must be imaginative in thinking of possible explanations. Identifications of meanings that were attributed by people who are studied must be made with due caution. And researchers' judgements as to which explanations are particularly good must be sound.

An important control that can promote satisfaction of these conditions is the scrutiny that work receives from other researchers. This scrutiny can take two forms.

The first form is direct responses to specific pieces of work. If a claim that an explanation is a good one is found not to be based on work of sufficient quality, other researchers may make that fact known. This is part of the normal process of academic work, either before publication through a system of peer review or in open discussion after publication.

The relationship of such scrutiny to satisfaction of the conditions for us to have a high level of confidence is sometimes direct and sometimes indirect. It is likely to be direct in relation to conditions that concern the acquisition and the basic analysis of evidence, such as the conditions that experiments must be conducted properly and that the use of statistical tests must recognize the scope for various types of error. It is likely to be indirect in relation to conditions that concern the psychological processes of researchers, such as the conditions that researchers must be imaginative enough, that they must identify attributed

meanings in a sensible way, and that they must exercise sound judgement. Any deficiencies in these latter respects would be identified indirectly through reviewers pointing out gaps in processes of reasoning, or proposing other explanantia for the evidence.

The second form that scrutiny can take is work on the explanatory capacities of common forms of analysis of the objects of study. Such work can prevent the inappropriate use of those forms of analysis to produce explanations. For example, an economic model may be argued to be unable to explain phenomena of certain types, such as asset-pricing anomalies or the effects of certain shocks on vacancies and unemployment.[52] To take an example from behavioural science, it has been argued that game theory, supplemented by other-regarding preferences, cannot be used to explain a certain significant behaviour, even though situations in which that behaviour arises can perfectly well be modelled in a way that makes it easy to call on the resources of game theory.[53]

8.3.5.2 The element of decision

We must ask whether there are adequate controls over the decisions that researchers take when they use the good explanation route, and whether those controls suffice to reduce the risk of a certain type of circularity to a tolerably low level. There are two types of decision that should concern us, decisions as to how to approach phenomena and decisions as to whether to assent to claims.

[52] Lewellen and Nagel, "The Conditional CAPM Does Not Explain Asset-Pricing Anomalies"; Shimer, "The Cyclical Behavior of Equilibrium Unemployment and Vacancies".

[53] Colman, "Love is not Enough: Other-Regarding Preferences Cannot Explain Payoff Dominance in Game Theory".

Decisions as to how to approach phenomena

Researchers will decide to approach phenomena in certain ways. For example, they may approach some economic phenomena by considering large-scale economic forces or by considering choices made by individuals. Or they may choose any one of a wide range of approaches to social phenomena, for example taking quantitative measurements, searching for causal relationships, focusing on institutions, focusing on culture, seeing people as social creatures of habit, seeing them as rational pursuers of their individual interests, or starting with detailed descriptions of phenomena in the words of members of the relevant societies.[54] We should not suppose that researchers find such decisions already made for them by the unvarnished facts of the world. If we were to follow Helen Longino's approach we would see researchers as having real choices, even if they did not notice that they were making choices. Longino sees researchers as selecting ways to characterize objects of study, where there are several options and researchers choose on the basis of the questions they want to answer. Since researchers are undoubtedly free to choose those questions, they have a certain freedom to choose how to characterize objects of study: they do not simply discover

[54] The wide range of approaches in the social sciences and the scope for a choice of approach to influence research are indicated by Della Porta and Keating (eds.), *Approaches and Methodologies in the Social Sciences: A Pluralist Perspective*. To take a narrower field, the range of qualitative approaches to the psychological analysis of what people say and how they say it is illustrated by Willig and Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*, chapters 4 to 9 (chapter 4: Wilkinson and Kitzinger, "Conversation Analysis"; chapter 5: Wiggins and Potter, "Discursive Psychology"; chapter 6: Arribas-Ayllon and Walkerdine, "Foucauldian Discourse Analysis"; chapter 7: Frosh and Saville Young, "Psychoanalytic Approaches to Qualitative Psychology"; chapter 8: Stephenson and Kippax, "Memory Work"; chapter 9: Hiles and Čermák, "Narrative Psychology".)

that they must be characterized in certain ways.[55] Decisions as to how to characterize objects of study may very well influence how researchers appraise explanations, and may therefore influence their judgements as to the quality of explanations of phenomena. And there is no guarantee that such influences will be in the direction of making judgements more accurate.

Concern about this type of decision merges into concern about a certain type of circularity. Once researchers have decided to approach some phenomena in a certain way, so that they handle the evidence in a certain way, that will steer them toward favouring some explanations over others, even though some of the other explanations would have appeared to be equally good if the researchers had decided to approach the phenomena in a different way and had therefore handled the evidence differently. The favourable verdicts that researchers reach on the preferred explanations are then likely to fortify them in their decision to approach the phenomena in the chosen way, completing the circle. At the extreme, a whole discipline might be built around approaching phenomena in certain ways, and researchers might exclude from their discipline anything that was not effectively handled using those approaches.[56]

The failing need not be as grave as that of deciding what to believe and then finding a route to the desired beliefs, because researchers may decide to approach phenomena in a particular way without first considering where that will lead. But there is a risk of unreasonably disregarding some explanations because initial decisions did not put researchers in a good position to see their merits. This

[55] Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, page 100.

[56] De Langhe, "Mainstream Economics: Searching Where the Light is", pages 140-142.

matters, because some unjustly neglected explanations might have been good enough to cast doubt on the worth of those that were favoured. The goodness of some neglected explanations might have shown, or at least suggested, that the favoured ones missed the point.

Cladistics will illustrate the importance of approaching phenomena in an appropriate way. The objective is to create family trees of the members of sets of species, on the basis of shared features that are regarded as derived from common ancestors. The species in a given set are the phenomena, and a family tree may serve as an explanation of their existence. The first stage is to group species according to certain shared features, and a choice of types of feature is a choice of how to approach the phenomena that will directly affect how researchers handle the evidence. If features that are not in fact good indicators of shared ancestry are chosen, any family tree that is constructed is likely to be inaccurate. A tree may appear to explain the existence of the observed species, but it will not in fact do so. Strict controls are therefore needed, especially given that researchers will come from traditions and fields of expertise which will incline them to make certain choices of features to identify. Fortunately, it is possible to identify controls.[57]

[57] The danger of making incorrect choices, and some controls, are discussed in Winther, "Character Analysis in Cladistics: Abstraction, Reification, and the Search for Objectivity". There does however remain some controversy about cladistics, and the safe course would be to take seriously what Winther says about the need for controls and their existence without committing oneself to his terminology of objectivity. On a more specific note, two controls that Winther discusses are of wider interest than the others. The first one is the requirement to identify causal structures that underlie observed features of organisms. The second one is the requirement to share and evaluate information across disciplinary boundaries. Winther discusses these controls on pages 151-152.

Cladistics is a rather specialized example. It would be reassuring to identify controls of general application that could reduce to a tolerable level the risk that researchers might miss good explanations because of prior decisions on how to approach phenomena, whether prior decisions of local application or commitments to large-scale paradigms.

We can identify one control. This is the diversity of outlook that can be found among researchers in some disciplines. Diversity gives a reasonable prospect that a wide range of ways to approach phenomena will be kept in play. This reduces the risk that explanations that should be considered will be overlooked because of decisions to approach phenomena in certain ways.

Diversity is not equally useful as a control in all disciplines. In disciplines that are low down the scale, such as physics and chemistry, the heterodox may be ostracized. Higher up the scale, diversity of outlook is more readily accepted. In economics, proponents of some approaches that are not perceived as mainstream and that may be critical of the prevailing orthodoxy can come to influence the evolution of mainstream economic thought.[58] There is indeed a view that heterodoxy can quite easily give rise to new orthodoxy.[59] And it is perfectly acceptable for a historian to be conservative or Marxist, or to adopt any one of a wide range of other positions.[60] There is also scope for new approaches to lead to fresh thinking about topics, thereby subjecting existing explanations to fresh scrutiny.

[58] Colander, Holt and Rosser, “The Changing Face of Mainstream Economics”.

[59] Davis, “The Turn in Recent Economics and Return of Orthodoxy”.

[60] For current prospects for Marxist approaches see Wickham (ed.), *Marxist History-Writing for the Twenty-First Century*. Conservative historians form a less easily definable school of thought than Marxists, and the label may fit individuals to widely varying degrees.

For example, new approaches to the study of history have developed in recent decades, leading to the wider adoption of perspectives other than those of the home continents of historians, an increase in the amount of work that is done outside the traditional framework of nation states, and increasing interest in the perspectives of people outside politically and economically dominant groups.[61]

Another danger is that researchers may think they have given a comprehensive explanation of some phenomenon when they have in fact given an explanation that is limited by their choice of approach. Helen Longino's discussion of different ways to approach the question of the influences that lead some people to exhibit certain types of behaviour brings out this risk.[62] She points out that while it is tempting to think that research can identify the causes of various sorts of behaviour, approaches that rely on genetics, on the study of social and environmental factors, on developmental systems theory and on neurology investigate different specific questions. They may for example investigate the extent to which genes affect behaviour, how the environment affects behaviour, how behaviour is expressed in individuals, and whether specific neural structures are associated with given types of behaviour. Such work may not answer big questions, such as the question of which factors are most significant as causes of human aggression.[63] The writers of newspapers and scripts for broadcast are the

[61] Sachsenmaier, *Global Perspectives on Global History: Theories and Approaches in a Connected World*, chapter 2.

[62] Longino, "Evidence in the Sciences of Behavior". There is a more extensive discussion of work on human behaviour in Longino, *Studying Human Behavior: How Scientists Investigate Aggression and Sexuality*. See also the much earlier discussion of research on sex differences, of assumptions made in that research and of influences on that research in Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, chapters 6 and 7.

[63] Longino, "Evidence in the Sciences of Behavior", page 254.

people most likely to overstate the breadth of conclusions from research, and it would be unwise for anyone to set much store by newspaper and broadcast reports of claims in academic fields. But it is also possible for researchers to forget that they may have provided only contributions in the general area that is covered by some big question, such as the question of the causes of aggressive behaviour, rather than providing answers, or even partial answers, to the big question itself. They may then overstate the significance of their work. The diversity of ways in which work may be done in the general area that is covered by a given big question is a useful reminder of this danger. But it may have to operate as a reminder to us just as often as to researchers themselves, because those who engage at a technical level with each researcher's work will tend to approach the relevant topic using the same general methods as the researcher.

Decisions as to whether to assent to claims

The second type of decision that should concern us is this. Once there are some candidate explanations laid out for consideration, researchers must decide which explanations are good enough to be endorsed, leading to assent both to their claims to explain and to at least some of the claims in their explanantia. Reassurance as to the quality of decisions may come from the skill and experience of researchers, and from researchers' patterns of thought being shaped by their awareness of a corpus that has itself been well-tested.[64] Such considerations do not give complete reassurance, but no better source of reassurance is available.

[64] The point about researchers' judgement being guided by a background is made in Lipton, *Inference to the Best Explanation*, pages 157-159, in the context of ranking explanations to identify the best one.

8.3.5.3 Identifying the better explanations

When researchers support claims that play explanatory roles by asserting that explanations in which they play those roles are better than explanations in which they have no such roles, special concerns arise.

One problem is that while some explanations might be the best ones out of those seriously considered, there might be other explanations that were inappropriately denied serious consideration, perhaps because of decisions to approach phenomena in certain ways, and yet other explanations that did not even come to the attention of researchers – the problem of unconceived alternatives that we noted in section 8.3.3.1. Here we must rely on researchers' disciplined and intelligent use of imagination and, in some disciplines, on researchers' diversity of outlook. In regard to the former, Peter Lipton pointed out that researchers do not blindly generate possible explanations. Their awareness of existing work helps them to come up with explanations that are likely to be worth considering.[65] It does not however follow that the problem of unconceived alternatives can be entirely solved by pointing to the role of existing accepted claims. Researchers might focus on the areas where good explanations were most likely to be found, and intensive work on those areas might turn up most of the candidates worth considering. But there would remain the risk that there were worthy candidates in other areas.[66]

[65] Lipton, *Inference to the Best Explanation*, pages 150-151. Lipton's argument may be compared with our remarks in section 8.3.3.2 on guidance provided by the corpus and paradigms.

[66] Compare the argument in Stanford, *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*, pages 40-41.

Another problem is that there might be something amiss in the order of preference between explanations. An explanation that is identified as superior to others may not in fact be superior, even among the explanations that are seriously considered. Here, Lipton offered a piece of reassurance that we noted in section 8.3.5.2. Explanations are ranked by reference to an existing background of theories.[67] If the theories have themselves been thoroughly tested and have proved their worth, it is likely that rankings will be at least approximately correct.

8.3.5.4 Formal epistemology

There is a large body of work that formalizes processes of belief formation and revision. This work sits within the discipline of formal epistemology, with its sub-disciplines of computational epistemology and epistemic utility theory. The work has applications in several of the areas that interest us, but its relevance to our main question is greatest in connection with use of the good explanation route. Its relevance in this connection springs from the fact that when researchers work toward accounts by non-deductive means, they must juggle uncertainties.

Formal epistemology will not address all concerns. Its precisely specified results mostly relate to idealized situations that are purged of the messiness of real research. Such results also mostly relate to disciplines in which hypotheses and claims are quantitative. It is no accident that examples are often drawn from physics. We cannot expect that

[67] Lipton, *Inference to the Best Explanation*, pages 157-159. Lipton's concern is not merely to establish that rankings of explanations are likely to be satisfactory, but that if they are satisfactory, that makes probable at least the approximate correctness of the relevant background.

precisely specified results in formal epistemology would often be straightforwardly usable in the humanities or the social sciences. There will even be quite a lot of work in the natural sciences in which it would be hard simply to use such results. Nonetheless, it is worth asking how results in formal epistemology might be brought to bear in order to control use of the good explanation route. We shall first make some remarks on the possibilities, then reflect on reasoning that it would not be practical to formalize sufficiently to make full use of the power of formal epistemology.

Norms

Work in formal epistemology can show the advantages of certain norms. For example, epistemic utility theory can be used to vindicate compliance with norms of probability theory. These norms constrain the assignments of credences across ranges of possibilities, and also show how to update credences when new evidence comes to light.[68]

We should not however hope to get all or even most of the reassurance we might like simply from the fact that claims have come to be accepted in compliance with norms that can be justified formally. Norms of probability theory are too general to give us much reassurance in the face of the scope for errors of types that are specific to disciplines. And there are times when the presence of formally definable properties does not in itself give us much reassurance. For example, it can be argued that the formally defined coherence of a set of hypotheses need not in itself be a sign that the members of the set are correct. Empirical work may be needed to see whether there is a general connection

[68] Pettigrew, "Epistemic Utility and Norms for Credences".

between coherence and correctness.[69] But on the optimistic side it can be argued that under certain conditions, an increase in the coherence of a body of evidence when a hypothesis is assumed does contribute to confirmation of the hypothesis.[70] Having said that, much depends on how confirmation is measured. There is an argument that in one model of confirmation, coherence between pieces of evidence (as distinct from coherence between evidence and a hypothesis) is not in itself an additional source of confirmation.[71]

Explanatory power

Researchers have a sense of explanatory power, and they may use that sense to rank explanations in order of preference. If they support claims by arguing that explanations in which those claims play explanatory roles are better than explanations in which they have no such roles, such rankings can have a direct effect on whether claims get to join the corpus. It would therefore be helpful to have rankings controlled by measures of explanatory power.

Work on such measures is at an early stage. It is so far limited to laying down conditions on quantitative measures of explanatory power, and it is not even clear that the most appropriate measures for all disciplines would have

[69] Morgan, "Achinstein and Whewell on Theoretical Coherence".

[70] Wheeler and Scheines, "Causation, Association and Confirmation". There is also the argument in Thagard, *The Cognitive Science of Science: Explanation, Discovery, and Conceptual Change*, chapter 6, that "coherence of the right kind leads to approximate truth" (page 81). In Thagard's view, coherence is of the right kind when researchers not only find a theory that best explains the evidence, but can also explain why the theory works (pages 91-99).

[71] Shogenji, "The Role of Coherence of Evidence in the Non-Dynamic Model of Confirmation".

to meet those conditions. It is also unclear that it would ever be practical to apply such measures to the variegated and complex explanations that are given in practice. But at least a start has been made.[72]

The identification of causes

The identification of causes is central to the construction of many explanations. One method that can be used to control identification is the method of graphical causal models, which are directed acyclic graphs that relate different variables. When probabilities for different values of variables conditional on the values of other variables are included, Bayesian and other probabilistic methods can be used, and the graphs are often called Bayesian nets. Graphical causal models can also help to identify confounding variables and sources of bias, thereby helping researchers to avoid mistakes. There are other methods that can be more suited to particular tasks, and that can likewise impose discipline on reasoning.[73]

Researchers should however not read too much into the deliverances of such methods. As we noted in section 5.3.2,

[72] Conditions that a measure should satisfy, and a measure that satisfies them, are set out in Schupbach and Sprenger, "The Logic of Explanatory Power".

[73] For an introduction to a range of techniques and their uses see Pearl, *Causality: Models, Reasoning, and Inference*. Several methods are discussed in Morgan (ed.), *Handbook of Causal Analysis for Social Research*. For the method of structural equation modelling see Kline, *Principles and Practice of Structural Equation Modeling*. Some methods are compared in the context of epidemiology in Greenland and Brumback, "An Overview of Relations among Causal Modelling Methods". For some reservations about the capabilities of techniques that use directed acyclic graphs, against the background of a broad welcome for their use, see Kincaid, "Mechanisms, Causal Modeling, and the Limitations of Traditional Multiple Regression".

it is possible to identify a relationship between two variables where the relationship has predictive power, without thereby showing that variations in the value of one variable cause variations in the value of the other. Thus we may be denied the favourable effects on our confidence that would go with the identification of causal relationships. It is therefore important to distinguish between the giving of causal explanations through the identification of apparent (but possibly illusory) structures of causes and effects, identifications that can be made by using directed acyclic graphs and comparable methods, and the justification of causal claims, which would require further work. There is also scope to change the nature of the problem by adopting a theory of causation that would allow causal relationships to be inferred from mathematical relationships.[74] We would however then need to consider whether the identification of causal relationships under such a conception would have favourable effects on our confidence that were comparable to the favourable effects of the identification of causal relationships under a more traditional conception.

Suppose that claims to explain that relate to causal explanations have come to be accepted. If the causal connections that are identified have been inferred using a method that formalizes the process of identification and is generally recognized to be sound, and if the causal claims have also been substantiated by other means, such as by the use of process tracing to set out detailed paths through which causes would have their effects, that should increase our confidence in the claims to explain.[75]

[74] Woodward, *Making Things Happen: A Theory of Causal Explanation*, chapter 7.

[75] For process tracing see Bennett, "Process Tracing and Causal Inference"; Mahoney, "The Logic of Process Tracing Tests in the Social Sciences".

There can of course be difficulties both in identifying all the variables that should feature as nodes in graphical causal models, and in quantifying probabilities. And it can be computationally demanding to establish the correct model from data, so it may be necessary to use special approaches that sometimes restrict the range of options considered. But despite such difficulties, graphical causal models have come to find application in both the natural and the social sciences.[76]

Efficiency

An important theme in computational epistemology is the efficiency of ways to arrive at claims that are correct or close to correct. Some methods are more efficient than others at leading researchers to converge on correct claims. Inefficient ways to converge on correct claims might lead them to perfectly good results in the end, but it would be odd to choose those ways when efficient ways were available.

We can go further. The use of efficient methods can in itself give confidence in accepted claims. If methods are efficient, that should limit the time it takes researchers to arrive at claims that are correct or close to correct. Then if work on identifying claims to make has gone on for a reasonable time, there is a good prospect that claims put forward for assent are by and large correct or close to correct. That should in turn reduce the number of claims that come to be accepted despite being far from correct, given that some

[76] For an example from biology which uses partially directed acyclic graphs see Le et al., “Inferring microRNA-mRNA Causal Regulatory Relationships from Expression Data”. The paper mentions computational challenges in sections 2.3, 2.4 and 4. For an example from economics see Li, Woodard and Leatham, “Causality among Foreign Direct Investment and Economic Growth: A Directed Acyclic Graph Approach”.

proportion of claims made that are far from correct will come to be accepted.

A particularly interesting line of enquiry has concerned the value of a preference for simple theories and models. Experience has lent some support to this preference, despite the lack of any guarantee that nature will be simple in any given respect. Some of the most successful theories and models have been very simple and elegant. Even when nature is complex, use of a simple approximation may confer more predictive power than use of an attempted full representation of nature's complexity, because it is difficult to estimate large numbers of adjustable parameters accurately on the basis of limited data that will contain some noise.[77] Computational epistemology offers an additional reason to prefer simple theories and models, at least in the context of reasoning from a growing body of evidence to a general conclusion. The new reason is that a preference for simplicity tends to make progress toward correct claims more efficient than it would otherwise be, with fewer false moves that need to be retracted. The idea can made precise for idealized research problems by stating Ockham efficiency theorems.[78]

Computer programs

A great deal of work has been done on getting computers to reason in ways that would allow them to solve problems

[77] Forster and Sober, "How to Tell When Simpler, More Unified, or Less *Ad Hoc* Theories will Provide More Accurate Predictions", section 2.

[78] A straightforward account is given in Kelly, "Simplicity, Truth, and the Unending Game of Science". A more technical presentation is given in Kelly, "Simplicity, Truth, and Probability". An extension to stochastic methods is given in Kelly and Mayo-Wilson, "Ockham Efficiency Theorem for Stochastic Empirical Methods".

of the types with which human researchers are confronted, whether or not by getting them to think in ways that are similar to human ways. Work has for example been done on how to get computers to reason abductively, to reason about intentions, and to prove mathematical theorems by thinking like human mathematicians.[79]

Such work is not yet at the stage at which its results could control the work of human researchers to any great extent by telling them that they ought to reason in the same ways as the most efficient computer programs. But we may expect that in due course this work will become a valuable control over the reasoning of researchers, especially when they use non-deductive routes to accounts. Moreover, the very act of writing computer programs that can do this kind of work helps researchers to describe some processes of reasoning and understand the limits of those processes. To the extent that human researchers use the same or comparable processes, this may lead to a better appreciation of ways in which results obtained by human researchers may sometimes need to be subjected to especially stringent checks.

Reasoning that is not in practice formalizable

There is a great deal of reasoning that it is not practical to formalize sufficiently to use results drawn from formal epistemology in ways that would make full use of their power. The complicated nature of the entities that are studied in the humanities and the social sciences, and the

[79] The wide scope of such work is indicated by the papers in Artikis, Craven, Çiçekli, Sadighi and Stathis (eds.), *Logic Programs, Norms and Action: Essays in Honor of Marek J. Sergot on the Occasion of His 60th Birthday*. A program to prove theorems by thinking like human mathematicians is described in Ganesalingam and Gowers, “A Fully Automatic Problem Solver With Human-Style Output”.

limited scope for quantification in those disciplines, together mean that researchers must use processes of reasoning that do not conform to ideal standards of logic and probability theory. Even within the natural sciences, researchers can fall short of ideal standards. Don Fallis points out that scientists do not in fact reason solely in the ways that Bayesian theory would prescribe, and goes on to argue that they should not reason solely in those ways.[80] And if natural scientists do not adhere strictly to Bayesian requirements, we can hardly expect social scientists or researchers in the humanities to do so. Moreover, given that a leading reason for researchers not being strictly Bayesian is the difficulty of quantifying degrees of belief and working out how to compute revised degrees, it is not likely that researchers in any discipline could adhere strictly to a Dempster-Shafer approach either. We may likewise have reservations about the extent to which proposed formal logics of abduction could adequately represent how researchers thought about real problems.[81] Similarly, it is hard to see real researchers as being in a position to ensure that they always follow the prescriptions of the AGM theory of belief change, or of any development of that theory.[82]

Inability to be purely Bayesian does not however mean that Bayesian methods have no roles to play. We have already noted the scope to use graphical causal models, which can take a Bayesian form. And there are plenty of other ways in which Bayesian methods can be used in both the natural and the social sciences.[83]

[80] Fallis, “Attitudes Toward Epistemic Risk and the Value of Experiments”.

[81] An example of such a logic is given in Meheus and Batens, “A Formal Logic for Abductive Reasoning”.

[82] For the AGM theory and some ways to develop it see Fuhrmann, “Theories of Belief Change”.

[83] For examples see the chapters in O’Hagan and West (eds.), *The Oxford Handbook of Applied Bayesian Analysis*.

There is also scope to work with Bayesian methods when it is not possible to give precise likelihoods.[84] In addition, the AGM theory of belief change and developments of it show how to map out options and their relative merits systematically. And ranking theory, which orders alternative claims according to degrees of belief or disbelief in them, also offers a way to regiment reasoning.[85] But sometimes there will not even be enough agreement among researchers about imprecise likelihoods, about all of the beliefs and relationships of implication that must be identified in order to apply theories of belief change rigorously, or about the degrees of belief or of disbelief that must be identified in order to follow the prescriptions of ranking theory. Then our confidence in accepted claims may be reduced, simply because researchers' processes of reasoning may not have been very tightly controlled.

Given the difficulty of regimenting some processes of reasoning, we may expect that researchers will sometimes use processes that are intermediate between those that conform to the ideals identified in formal epistemology and those that we use to solve problems in everyday life, where we simplify enormously and focus on one or two salient considerations. In the absence of complete regimentation it is perfectly rational to do so, and doing so confers the enormous benefit that it allows work to be produced. The temptation to use processes of reasoning that have quite a lot in common with everyday processes is likely to be greatest in disciplines high up the scale, so we shall concentrate on them.

Everyday processes can be just as good as more sophisticated processes, as shown by the work of Gerd Giger-

[84] Hawthorne, "Bayesian Confirmation Theory", section 5.

[85] Spohn, *The Laws of Belief: Ranking Theory and its Philosophical Applications.*

enzer and Daniel Goldstein.[86] But the examples given by Gigerenzer and Goldstein are of simple choices that are made by reference to simple rules. The historian or the sociologist, trying to make sense of a mass of evidence and well aware of the complicated nature of the objects of study, would not appear to be confronted with such simple choices. Nor would a historian or a sociologist be likely to rely on anything as simple as picking out features of a situation to make central to explanations by reference to a single indicator of importance, by analogy with the cue-ranking system that is used at stage 2 of the "take the best" algorithm. Nor would she be likely to engage in one-reason decision making, in which decisions on what to say would be made for a single reason rather than by weighing up several reasons.[87] Nonetheless, we can expect certain features of everyday reasoning to be carried across to the practice of academic disciplines. Researchers will have a sense of which signs commonly indicate that a factor in a situation will have explanatory importance, and they will pay special attention to factors that exhibit those signs. Researchers who work with copious sources will also have to be selective in their consideration of evidence, so that some factors which might have influenced their decisions as to what to say will simply be ignored.

The effect on our confidence will depend on our view of the judgement of researchers as to indicators of explanatory importance, and on our view of their judgement as to what to ignore. The better their judgement appears to be, the greater confidence we may have in their decisions on which claims to make and on whether to assent to claims.

[86] Gigerenzer and Goldstein, "Reasoning the Fast and Frugal Way: Models of Bounded Rationality".

[87] For "take the best" see Gigerenzer and Goldstein, "Reasoning the Fast and Frugal Way: Models of Bounded Rationality", pages 653-654. For one-reason decision making see pages 662-663.

8.3.6 Conclusion

Use of the good explanation route, endorsing explanations when they are good enough, is not guaranteed to give us confidence in the corresponding claims to explain or in claims that are supported by their playing explanatory roles. Searches for candidate explanations may not be wide enough, paradigms may misdirect searches, scrutiny may be inadequate, and the necessity of using processes of reasoning that cannot be fully regimented within any of the frameworks offered by formal epistemology raises the prospect of accidentally using processes that are defective in respects that matter. Nonetheless, the good explanation route does work. The willingness of researchers to endorse explanations because they consider them to be good has led disciplines to flourish. And an important reason is likely to be that any one researcher's decision to endorse an explanation is open to scrutiny by others. We shall discuss the independent appraisal of work in some detail in section 9.4.

Chapter 9

Creators of Accounts

9.1 The complexity of the world

We have made many references to how disciplines are practised and to their norms. In this chapter, we shall turn to researchers in their own right. We shall consider both individual researchers and the communities within which they work.

The world is very complex, and there is much to discover. There are several implications for the work of researchers. We shall list them here, and discuss them in subsequent sections of this chapter.

Researchers must work in groups of various degrees of cohesion and permanence. We shall consider the effect of this on levels of support for claims that researchers make and to which they assent. Even if we had reason to think that the effects of ways of working would be minimal in the long term, because researchers would eventually converge on the same claims and all claims that were not well-

supported would eventually be exposed as such, the short term would still matter because currently perceived levels of support for accepted claims should affect our current confidence. (See section 9.2.)

There is likely to be a great deal of work that is relevant to the work of any given researcher or group of researchers, but that has not been done by that researcher or group. Researchers and groups must obtain information from others, and hence must rely on testimony. (See section 9.3.)

The need to rely on testimony gives a special role to the independent appraisal of researchers' work. Such appraisal can reassure researchers that they can sensibly rely on testimony that is obtained from publications. Appraisal is formalized in systems such as peer review. We shall consider what degree of control such systems provide. (See section 9.4.)

New technologies affect both the management of information and the ways in which researchers can work together. We shall consider how such developments affect researchers' ability to avoid over-estimating levels of support that claims enjoy. (See section 9.5.)

The division of labour that the complexity of the world requires leads to demands that researchers should possess certain epistemic virtues in order that others may rely on their work. We shall consider the roles of various virtues. (See section 9.6.)

9.2 Work in groups

9.2.1 The importance of groups

The complexity of the world means that in many fields, progress can only be made by groups of individuals who between them have the right expertise. There are other benefits of work in groups beyond simply making it possible to conduct research. These include improvements in the quality of work, making it more likely that researchers will be aware of relevant work already done, and the provision of effective training for new researchers.[1] It is not surprising that work in groups has grown in importance in recent decades, whether or not such benefits explain all of the development of work in groups.[2]

It is also possible to argue for the importance of work in groups, and more broadly in disciplinary communities, on grounds of principle, rather than merely by noting practical benefits. Helen Longino has done so. She sees it as vital for researchers not to work alone, but to interact with others in order to neutralize the potential influence of personal idiosyncrasies. She thereby comes to see work in groups not as an optional variant on some more fundamental process of observation and reasoning by an individual, but as an independently valid way to work.[3] This is an important

[1] Wray, “The Epistemic Significance of Collaborative Research”, pages 156-158.

[2] For the growth in importance see Wuchty, Jones and Uzzi, “The Increasing Dominance of Teams in Production of Knowledge”. For a discussion of possible explanations of the development of patterns of work in groups see Wray, “The Epistemic Significance of Collaborative Research”, sections 4 and 5.

[3] Longino, *The Fate of Knowledge*, pages 106-107. See also the earlier exploration of science as a social practice within which values play a critical role in Longino, *Science as Social Knowledge: Values*

line of thought, but we shall not pursue it here since it is evident that work in groups is in any case the norm in many disciplines. We must answer our main question in the light of that fact, regardless of the reasons why it has become the norm.

The workings of groups have become an important theme in epistemology in recent years, with the study of group cognition, of epistemic group agents, and of the nature of evidence (whether evidence gathered by a single group or by agents who cannot be regarded as unified in a single group).[4] But work in groups has been central to many disciplines for a long time. While new epistemological work will undoubtedly come to illuminate some of the issues that concern us, the issues themselves and their significance for our main question can be set out without waiting for that work to reach conclusions that might aspire to finality.

The fact that work is done in groups may affect both the process of making and assenting to claims and our confidence. Our confidence may be affected because groups are a powerful tool for the imposition of norms, the sharing of ideas, and the imposition of orthodoxy. We shall discuss these influences in sections 9.2.2, 9.2.3 and 9.2.4 respectively. The influences may affect solitary researchers as well as members of groups, because even a solitary researcher is heir to a tradition of thought and is aware of current work by others.

and Objectivity in Scientific Inquiry, chapters 3 to 5.

[4] Theiner, Allen and Goldstone, "Recognizing Group Cognition"; de Ridder, "Epistemic Dependence and Collective Scientific Knowledge"; Palermos and Pritchard, "Extended Knowledge and Social Epistemology"; Kerr and Gelfert, "The 'Extendedness' of Scientific Evidence".

9.2.2 The imposition of norms

The observance of norms is essential to the production of worthwhile work. Fortunately, it is perfectly possible for norms to be imposed. The imposition may not be felt because researchers may have internalized the norms, so that the existence of the apparatus of imposition would only be recognized through counterfactuals: if a researcher were to violate the norms, there would be an effective response.

At the most general level, imposition may take the form of making sure that everyone respects the culture of the discipline as a whole, with norms being an important part of that culture. The importance of such a culture has been emphasized by Markus Arnold. He speaks of the Wissenschaftskultur of a discipline, a discipline-culture that encompasses traditions, practices, ethical norms, and the correct use of methods of communication. He explores the ways in which such a culture can hold a discipline together, keep it on track and influence its direction, and a range of social and intellectual implications of the particular cultures that disciplines possess.[5]

At a more specific level, those who work together may enforce observance of detailed technical norms, for example by pointing out occasions on which instruments may have been used carelessly or conclusions may have been drawn without the benefit of results from a suitable control group.

Finally there is the norm that researchers should work and interact in ways that help to make collaboration effective, so that the benefits of division of labour and of the sharing of expertise can be reaped. Some of these ways have been

[5] Arnold, “Disziplin und Initiation: Die kulturellen Praktiken der Wissenschaft”.

set out by Paul Thagard.[6]

9.2.2.1 Accountability

There is a disadvantage to work in groups that bears directly on the quality of work done. There may be a loss of individual accountability, reflecting the fact that work done can very easily, and arguably correctly, be seen as the work of a collective agent rather than as the sum of pieces of work done by individuals.

There are some obvious dangers. An error may be hard to attribute to an individual; the work of some members of a group may accidentally be used by other members of the group in ways that are inappropriate because the users are unaware of all relevant technicalities, while no-one may reasonably be blamed for such accidents; and when one member of a group is challenged, he or she may find it all too easy to disclaim responsibility for mistakes made by other, perhaps unidentified, members of the group.

There is also scope for a subtle perverse incentive, which has been identified by K. Brad Wray. Members of a group may try to stop the group from recognizing problems with the work it has done, lest they as individuals be blamed.[7] Then published results may be inadequately supported by evidence, and that very fact may be hidden from the reader because defects in the work done will not be acknowledged publicly.

[6] Thagard, "How to Collaborate: Procedural Knowledge in the Cooperative Development of Science". Thagard is concerned with collaborative relationships generally, including but not limited to work in research groups.

[7] Wray, "Scientific Authorship in the Age of Collaborative Research", section 4.

Given that these risks exist, it is particularly important for members of a group not to let standards slip. They must individually consider whether they abide by all relevant norms and whether they exhibit all relevant epistemic virtues. They must not just assume that everything is in order, merely because that is the optimistic view of the group as a whole.

9.2.3 The sharing of ideas

The sharing of ideas is likely to be straightforwardly beneficial. Different members of a group will have different skills, will be aware of different existing claims within the corpus, and will have different ways of looking at problems. Progress will therefore be promoted, and some errors will be detected quickly. The same may be said about the sharing of ideas of groups as wholes with other groups.

Personal contact can be important in itself. It might seem that ideas could be shared by writing and reading papers just as effectively as by working alongside other people. That may sometimes be so, but it is not always so. In particular, skill in the use of techniques may best be acquired by working alongside other researchers, even though the techniques can be made explicit in papers and are to that extent unlike skills that cannot sensibly be made explicit, such as the skill of riding a bicycle. An example of a skill that is best acquired by working alongside others is the skill of using Feynman diagrams.[8] There is also a possibility that is intermediate between reading about methods and working alongside experienced researchers,

[8] Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics*, pages 167-169.

which is to watch videos of experimental procedures that have been published online.[9]

Turning to general abilities, while skills of assimilating new information, seeing relationships between pieces of information, making new inferences and appraising accounts can be developed to some extent by studying accounts that are already current, they are also largely developed by practice alongside others who are already highly skilled. And it is controversial whether what is learnt when such skills are acquired could be reduced to propositional information.[10]

It might seem that the benefits discussed here would relate only to the rate at which new claims could be reached, so that there would be little or no relevance to the question of control over the acceptance of claims. But in fact there would be considerable relevance to that question, in two ways. First, a wide range of skills, wide awareness of the contents of the corpus and the use of a range of different ways of looking at problems all contribute to the devising of tough tests of claims. Second, if claims are generated quickly, that can increase the incidence of rivalry between claims. Any claim with a rival is put under pressure, and that is likely to make the testing of claims more effective than it would otherwise be.

We should however consider the ways in which consensus may be reached when disagreement has emerged, or when some researchers in a group have not formed opinions

[9] Videos are for example published in the *Journal of Visualized Experiments*, http://www.jove.com.

[10] Newman, "EMU and Inference: What the Explanatory Model of Scientific Understanding Ignores", is a recent paper that takes a position on the controversy. Sections 5 to 7 of that paper set out several skills, with particular importance being attached to the skill of forging links between pieces of information by making inferences of certain types.

of their own. There is a risk that an eventual consensus may be unduly influenced by members of a group whose prestige outruns their expertise. One reassuring point here is that mathematical modelling of the process of reaching consensus suggests that so long as an appropriate balance of mutual respect is preserved, members of a group collectively give equal weight to different members' views.[11] Another reassuring point is that under certain conditions, there will be progress toward a consensus that is at least approximately correct.[12] Moreover, such results can be extended to reasonably complex situations, and in ways that illustrate the benefits of exchanges of views in promoting convergence on a correct consensus.[13]

Such results are however obtained for mathematical idealizations that simplify real-world situations. The need to pay attention to this point is underlined by the existence of tempting and seemingly intuitive extensions of such

[11] Hartmann, Martini and Sprenger, "Consensual Decision-Making Among Epistemic Peers", sections 3 and 6. For further discussion of the importance of the weighting of views and how weights might sensibly be established see Martini, "Consensus Formation in Networked Groups".

[12] Hegselmann and Krause, "Truth and Cognitive Division of Labour: First Steps Towards a Computer Aided Social Epistemology"; Hegselmann and Krause, "Deliberative Exchange, Truth, and Cognitive Division of Labour: A Low-Resolution Modeling Approach". There is substantial overlap between these two papers, and the second one is later, but the papers listed in the following note refer to the first one.

[13] Douven and Riegler, "Extending the Hegselmann-Krause Model I"; Riegler and Douven, "Extending the Hegselmann-Krause Model II"; Riegler and Douven, "Extending the Hegselmann-Krause Model III: From Single Beliefs to Complex Belief States". The last paper notes the interesting result that when epistemic agents exchange views with one another and give some weight to one another's views, this can actually slow down their progress toward getting reasonably close to correctness, even though such interaction may help them to get closer to correctness in the end than they otherwise would (section 5).

results that turn out to be incorrect.[14] Moreover, there is an argument that the most popular model of the process of reaching consensus, the Lehrer-Wagner model, is not a good model when the goal is to reach correct answers to factual questions.[15] This is however not an argument that conclusions reached in ways that fitted the model would be incorrect. It is only an argument that epistemologists should construct a better model.

9.2.4 The imposition of orthodoxy

Work in groups can be a route by which the orthodoxy of a research community comes to be imposed on individuals, through pressure to conform. The imposition of orthodoxy has its advantages. People cannot work together unless their approaches have a good deal in common. The imposition of orthodoxy can also help to bring eccentric members of groups to work in agreed ways, instead of wasting everyone's time by working in ways that are unlikely to be productive.

Despite these advantages, there is a trade-off to make. Agreement on how to work and agreement that certain claims should be regarded as well-established facilitate not only work in groups, but also the specialization of members of groups. They therefore allow great increases in productivity. But there may be a significant loss of diversity of approach.[16] There can be two adverse effects. (These adverse effects may arise whatever the source of the loss

[14] Kurz and Rambau, "On the Hegselmann-Krause Conjecture in Opinion Dynamics", sections 1 and 5.

[15] Martini, Sprenger and Colyvan, "Resolving Disagreement Through Mutual Respect".

[16] De Langhe, "The Division of Labour in Science: The Tradeoff Between Specialisation and Diversity".

of diversity. But agreement of the type that facilitates work in groups is an obvious source.)

The first adverse effect is that work that would in fact be productive, but that would not fit in with mainstream beliefs or ways of working, may be met with hostility or at best neglected. Examples can be given. When Dan Schechtman reached his conclusions on quasicrystals in the early 1980s, there was extensive hostility and the head of his group asked him to leave. (The hostility did however abate once his results were replicated.)[17] If we go back to the early and middle decades of the twentieth century, the long and hard road to general assent to continental drift serves as an example of how the grip of existing views on appropriate methods, and not merely views on theories, can delay recognition of the merits of new ideas.[18]

This adverse effect may reduce our confidence in claims that are already accepted, even though it relates to the difficulty of getting general assent to new claims. If there is openness to new claims which might challenge existing claims, that can increase our confidence in accepted claims. Either they

[17] Jha, "Dan Schechtman: 'Linus Pauling Said I Was Talking Nonsense'". For other examples see Campanario, "Rejecting and Resisting Nobel Class Discoveries: Accounts by Nobel Laureates". Campanario's paper is based on rejections of papers by journals. He acknowledges (on page 558) that not all rejections are to be explained by inappropriate resistance, although many rejections may well be so explained.

[18] Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science*. Oreskes argues that some earth scientists rejected continental drift because it did not fit with their beliefs about how earth science should be conducted (page 6). The beliefs about how to work were however beliefs with substantive content. In particular there was the uniformitarian belief that while the Earth might change in detail, large-scale patterns remained constant. This was, for the earth scientists concerned, a belief about how to work, not just a factual claim (pages 178-179).

will have been exposed to challenges and survived, or they will have escaped challenge despite not being protected by hostility to new claims. Conversely, hostility to new claims can reduce our confidence generally because we may fear that claims already accepted are unduly protected. A special form of this danger relates to ways of working rather than specific claims. If there is too much conformity of thought, researchers may neglect new approaches that would uncover new claims which might challenge existing accepted claims. There is a valuable role for mavericks who, while not necessarily rejecting currently accepted claims, are keen to try new approaches that others neglect.[19]

The second adverse effect of a loss of diversity is that diversity has advantages in debate. Diversity of view can keep up the pressure to justify orthodox positions. It can strengthen orthodox positions if they really are correct, demolish them if they are incorrect, or contribute to the formulation of new positions that combine correct elements drawn from both orthodox and heterodox views. Arguments for the existence of this benefit of diversity may draw on the work of John Stuart Mill.[20] In addition, dissent can highlight data and insights that would otherwise be neglected.[21]

We do however return to a version of the trade-off that we mentioned above. Conformity to standard basic assumptions and standard ways to think about the world allows great increases in productivity, and those increases

[19] Weisberg and Muldoon, "Epistemic Landscapes and the Division of Cognitive Labor". Some of the arguments in that paper have however been criticized: Alexander, Himmelreich and Thompson, "Epistemic Landscapes, Optimal Search and the Division of Cognitive Labor".

[20] Mill, *On Liberty*, chapter 2.

[21] Solomon, "*Groupthink* versus *The Wisdom of Crowds*: The Social Epistemology of Deliberation and Dissent", Abstract and section 3.

will be unavailable if a community of researchers is largely made up of open dissenters. But those who dissent privately while being outward conformists will not make debate benefit from diversity. It is possible to reach the strange position in which many researchers privately dissent from the reigning assumptions and approaches, while there is still an official consensus that those assumptions and approaches are correct. Such a consensus is preserved at least partly because the existence of some consensus or other greatly increases productivity. But eventually, the public consensus may very well break.[22]

To the extent that we are concerned about a loss of diversity, we may derive some reassurance from any evidence that researchers consciously try to take seriously the points of view of others who disagree with them, whether the others work within their groups or elsewhere.[23] We can also gain reassurance from studies which suggest that willingness to take note of what others say helps individuals to reach correct conclusions (so long as the probability that a person consulted will be correct exceeds 0.5), that a reduction in social influence on individual opinion without its elimination can be counterproductive (so that a high level of social influence is not necessarily a bad thing), and that only temporary diversity of views on any given question is needed.[24]

There is another risk to consider. New claims may come to be accepted without adequate support because of

[22] De Langhe, "To Specialize or to Innovate: An Internalist Account of Pluralistic Ignorance in Economics".

[23] Compare the duties of toleration that are discussed in Straßer, Šešelja and Wieland, "Withstanding Tensions: Scientific Disagreement and Epistemic Tolerance", sections 5.2 and 6.

[24] For the first two points see Zollman, "Social Structure and the Effects of Conformity". For the third point see Zollman, "The Epistemic Benefit of Transient Diversity".

groupthink.[25] The members of a group may be concerned to promote harmony. Ready assent to ideas put forward by members is likely to lead to harmony, while challenge may all too easily lead to conflict. Groupthink arises when the desire for harmony predominates. To the extent that claims avoid challenge within groups, our confidence must be reduced unless it is clear that claims are challenged by researchers outside the groups that make them. But that source of reassurance may be unavailable. A topic may be so specialized that only one group investigates it in depth.

We should not suppose that groupthink is always a serious problem. Researchers can be perfectly capable of maintaining a critical stance toward the work of their colleagues. There is even some evidence that members of a group who identify strongly with the group may be more inclined than other members to break a consensus by pointing out problems.[26]

Finally, even when there is no evidence of damage from the unjustified exclusion of alternative views or from groupthink, there is another concern. The fact that researchers in a group have worked together successfully and have fully and freely concurred in the group's conclusions does not show that there is a fully informed consensus on all points. A group can function perfectly well, even when each individual researcher only has a limited understanding of the topic and the work done.[27] The boost to our confidence that would be given by there being a fully informed consensus on all points may therefore be unavailable.

[25] For a recent survey of work on groupthink see Rose, "Diverse Perspectives on the Groupthink Theory – A Literary Review".

[26] Packer, "Avoiding Groupthink: Whereas Weakly Identified Members Remain Silent, Strongly Identified Members Dissent About Collective Problems".

[27] Andersen, "Joint Acceptance and Scientific Change: A Case Study".

9.3 Reliance on testimony

The need to make use of information obtained by others is the need to rely on testimony. This need arises both within groups and across a discipline's community as a whole. Within groups, researchers make use of results obtained by other members of the group. Across a discipline's community, researchers cite the work of others in order to make their own arguments. It would be impractical for researchers to check all of the work on which they relied. And work may well lie outside the areas of expertise of researchers who rely on it. If the testimony of other researchers might be unreliable and the unreliability might go unnoticed, that could undermine control both over the making of claims and over researchers' decisions on whether to assent to them.

We may consider how to regard testimony in academic disciplines. On the one hand research is supposed to concentrate on finding out how the world is, independently of the psychological states of researchers. This makes it very tempting to regard each piece of testimony as something to be evaluated on its own merits by reference to what it claims and the evidence for those claims. That view would have much in common with the statement view of testimony which Jennifer Lackey has put forward in relation to testimony generally.[28] On the other hand there are views that emphasize the social aspect of testimony, and that do so in ways that seem to make this emphasis directly relevant when researchers must put their trust in the expertise of others. Sanford Goldberg sees the process of forming beliefs on the basis of testimony as not limited to what goes on in the mind of the person who receives

[28] Lackey, *Learning from Words: Testimony as a Source of Knowledge*, chapter 3.

testimony, but as including what goes on in the mind of the testifier.[29] Benjamin McMyler takes an equally social view of testimony.[30] McMyler's approach is, as he sets out, particularly relevant to the provision of testimony without the argument that would allow the audience to bypass the role of the testifier and consider the argument directly.[31] This failure of the testifier to provide an argument has similar effects to those of the inability of researchers to appraise arguments when they have to rely on testimony from outside their own areas of expertise.

Our enquiry does not require us to decide on the proper view of testimony. But we do need to recognize the importance of both the view of testimony that emphasizes its content and the view that emphasizes the social aspect. On the one hand, testimony that is not well-supported by detailed argument should be disregarded, both because it is inherently suspect and because any researcher who borrows results but has some doubts about what they mean or about their reliability can only have those doubts assuaged by considering how the results were obtained and what the support for them might be, or at least taking advice on those points from suitable experts. On the other hand, reliance on testimony without studying the arguments that support its content does need to be reasonably safe, because in practice testimony will often have to be used without careful study.

We mention testimony here by way of introduction to the following sections, in which we discuss how some practices of researchers, the tools available to them and their standards

[29] Goldberg, *Relying on Others: An Essay in Epistemology*, page 80.

[30] McMyler, *Testimony, Trust, and Authority*, chapter 2. See in particular McMyler's comments on the speaker's acceptance of epistemic responsibility on pages 68-70.

[31] McMyler, *Testimony, Trust, and Authority*, pages 58-59.

of conduct may affect our confidence. Sometimes these factors bear directly on the quality of fresh work. But sometimes the influence that matters is on the quality of work done in the past, where testimony derived from that work supports fresh claims. We must ask whether the process by which those supporting claims originally came to be accepted was sufficiently well-controlled.

A third case arises when the influence that matters is on the use that current researchers make of testimony. We must ask whether they are careful enough about reliance on claims that came to be accepted in the past. If they just assume that work done in the past was of a high enough standard, and if testimony derived from that earlier work plays a significant role in making the case for fresh claims, that may reduce our confidence in the fresh claims. Our confidence should also be reduced by any evidence of carelessness in the selection of sources of testimony, for example by evidence of the selection of testifiers without careful consideration of their expertise.[32]

[32] There are some formal results on the effects of different ways to select sources of testimony, for example by reference to expertise or by reference to ease of access to sources: Mayo-Wilson, "Reliability of Testimonial Norms in Scientific Communities". But these results apply to idealized situations, they concentrate on the accuracy of the information immediately obtained rather than on the effects on work that uses it (although the use of inaccurate testimony is obviously likely to have a bad effect on work that uses it), and they do not decisively favour one way of selecting sources over all others. We shall therefore limit ourselves to the general point that carelessness is disturbing.

9.4 Independent appraisal

9.4.1 Appraisal as a control

A vital control over assent to claims is that claims should be appraised by researchers who were not involved in making them. Such appraisal may take place before or after publication. Before publication, there is peer review. After publication, appraisal may range from comments on blogs, through book reviews and papers that respond to other papers, right up to detailed technical studies of exactly how work was carried out.[33] Appraisal at any stage may involve the consideration of several aspects of a piece of work, including relationships to background literature, the type of study, specific methods used, statistical computations, and possible commercial influences.[34]

Criticism is an essential part of appraisal, and there are conditions for criticism to be effective. Conditions may be formulated in various ways. One formulation has been provided by Helen Longino. She sets out and elaborates on the need for suitable venues for debate, for responsiveness to criticism (which she calls uptake of criticism), for public standards, and for an equality of authority that is tempered by respect for manifest differences of talent and expertise.[35]

[33] For an example of a detailed technical study see European Food Safety Authority, “Final Review of the Séralini *et al.* (2012a) Publication on a 2-Year Rodent Feeding Study with Glyphosate Formulations and GM Maize NK603 as Published Online on 19 September 2012 in Food and Chemical Toxicology”, and particularly the Annex.

[34] For an outline of the range of ways in which a paper may be investigated, set in the context of medicine but to a large extent generalizable to other disciplines, see Greenhalgh, *How to Read a Paper: The Basics of Evidence-Based Medicine*.

[35] Longino, *The Fate of Knowledge*, pages 129-134. We may

But even when such conditions are satisfied, we may still have some concerns.

The first concern is that independence is a matter of degree. Even the harshest critic of a piece of work must share some presuppositions of the discipline, and probably most of them, in order to engage with the work productively. Fortunately, a revolt against the main presuppositions of a discipline is only rarely a source of progress.

A second concern is that appraisal may be influenced by rhetoric in the work being appraised or in the general context within which contentious points are debated, so that appraisal does not conform to the ideal of a wholly detached consideration of the evidence and the argument. The dangers are obvious in disciplines such as history, but the phenomenon can arise as low down the scale of disciplines as evolutionary biology.[36]

A third concern is that many pieces of work are never fully appraised. A claim may not be of immediate significance, and researchers who might appraise it may have more important uses for their time and energy. It may then come to be accepted by default, especially if it is contained in a paper by a reputable author that is published in a reputable journal. Others may then come to rely on the claim. It may be examined more carefully if it is to be used in some important work, but that cannot be guaranteed. It may even be used in several pieces of work, which are then used in other pieces of work, without the track back to

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compare these conditions with ones that have been developed by Roger Cooke for arriving at a rational consensus between experts, although Cooke's conditions tend to be cited in connection with special tasks, such as the appraisal of risk: Boumans, "Model-Based Consensus", section 3.3.

[36] Barahona and Cachón, "The Rhetorical Dimension of Stephen Jay Gould's Work".

the original work's being noted in papers that are at two or more removes from it. There is therefore a risk that claims which originally came to be accepted by default, and which may have come to be accepted without adequate support, will play roles in the processes by which several members of the corpus come to be accepted. Then the support for those claims may also be inadequate.[37] And if claims can get into the corpus without adequate support, it is likely that some incorrect claims will get in.

A fourth concern is the possible detrimental effect of multiple independent testing in areas of research that are popular. It may seem odd to be concerned about too much testing for an effect, but the problem is that if there are many tests for an effect that does not really exist, there is a reasonable probability that some tests will give positive results by chance. If the negative results do not get much attention, the positive results may be taken to show that the effect is real.[38]

It is unfortunately easy for negative results not to get much attention. It can be difficult to get journals to publish reports of null results, that is, reports of the absence of interesting outcomes of experiments. If there are several studies of the same phenomenon, and only the ones with interesting outcomes get published, the overall effect will be a false impression of the likelihood of certain outcomes.

[37] Compare the remarks of William Kingdon Clifford on the dangers of passing on to posterity beliefs that have not been examined carefully enough: Clifford, "The Ethics of Belief", section 1, pages 168-172.

[38] Pfeiffer and Hoffmann, "Large-Scale Assessment of the Effect of Popularity on the Reliability of Research". For issues that arise when multiple studies are reported together (for example, studies of the same phenomenon conducted in different ways or tests of a range of hypotheses), see Schimmack, "The Ironic Effect of Significant Results on the Credibility of Multiple-Study Articles".

Even a meta-analysis of studies can miss this. There are methods to estimate biases against publishing reports of null results and to try to correct for their effects, but the problem remains a real one. It has been recognized as serious enough to justify the creation of journals that make a point of publishing reports of null results.[39]

Two controls operate around the time of publication and help to reduce the risk that claims will come to be accepted without proper appraisal. The first control is the traditional form of peer review before publication. The second one is open comment, which may take place either before or after publication. We shall now consider the effectiveness of these controls, before noting the control that is provided by attempts to repeat work that has been reported.

9.4.2 Traditional peer review

Traditional peer review performs two functions. It checks the quality of work that is submitted for publication against a standard. It also chooses between works that meet the standard, so as to allocate the limited number of places that are available for peer-reviewed publication.

In practice, the second function can easily come to subsume the first. The number of opportunities to publish peer-

[39] For a study of this type of bias see Dwan et al., "Systematic Review of the Empirical Evidence of Study Publication Bias and Outcome Reporting Bias". For examples of methods to detect bias and to estimate its effects see Pfeiffer, Bertram and Ioannidis, "Quantifying Selective Reporting and the Proteus Phenomenon for Multiple Datasets with Similar Bias"; Simonsohn, Nelson and Simmons, "*p*-Curve and Effect Size: Correcting for Publication Bias Using Only Significant Results". For examples of journals that make a point of publishing reports of null results see the *All Results Journals*, http://www.arjournals.com/.

reviewed work falls well below the number of pieces of work that meet the standard. This is to be expected. In any given area of work, there will only be a modest pool of people who are competent to review the latest research, and those potential reviewers will also need to spend time on their own work. (This constraint is however not so severe if journal editors and publishers are content for peer review to be conducted inadequately. We shall shortly come to a reason why they might be content for that to happen.)

An editor will therefore apply a preliminary check to submissions, and may decide not to send for review a substantial proportion of them. The survivors will go to review, and the reviewers will send their comments back to the editor. The editor will then decide which pieces of work not only meet the standard that reviewers impose, but are also important enough to merit places in the relevant journal. There may be additional stages before a final decision is made. A common additional stage is to ask for a piece of work to be revised and resubmitted.

We should bear in mind that there is both a constraint based on a standard and additional rationing. Only the constraint that work must meet a standard is directly relevant to our confidence. But there are two ways in which additional rationing can be indirectly relevant.

The first way is this. Competitive pressures can encourage researchers to make their results seem more exciting than is justified by the work they have done, so as to increase the probability that their papers will be published in prestigious journals.[40] If any such tendency is not controlled by rigorous appraisal, exaggerated claims may come to be published, and may then come to be accepted at least partly because they have been published in peer-reviewed journals.

[40] McCook, "Is Peer Review Broken?"

The second way is this. If researchers only take much note of work that is published in peer-reviewed journals, and if work gets excluded even though it meets the appropriate standard, then work that would have helped researchers to discover problems with other work may go unnoticed because it did not get published in that way. The other work may then not be subjected to sufficiently searching criticism.

We should also ask whether peer review in fact ensures that claims which are contained in work that is endorsed by reviewers do enjoy adequate support. It would be too much to expect that nothing should slip through, but we may reasonably ask that the process should usually detect and eliminate claims which do not enjoy adequate support.

There are some grounds for concern. Reviewers do not appear to spend much time looking at papers, and when two or more reviewers report on the same paper, their views can be markedly different.[41] Those open-access models of publishing in which authors pay fees to have papers published have also given rise to concern because journals have financial incentives not to reject papers, although some of the journals in which this seems to be likely to lead to the publication of work of poor quality do give additional indications of being disreputable.[42] Subscription-based journals may also publish papers they should not publish. They too have a financial incentive, in their case an incentive to accumulate a large stock of published papers so as to increase their income from subscriptions and from fees for access to individual papers.[43] Here we have a reason

[41] Seidl, Schmidt and Grösche, "The Performance of Peer Review and a Beauty Contest of Referee Processes of Economics Journals", sections 1.3.1 and 1.4.

[42] Bohannon, "Who's Afraid of Peer Review?"

[43] Eisen, "I Confess, I Wrote the Arsenic DNA Paper to Expose Flaws in Peer-Review at Subscription Based Journals".

why journal editors and publishers might be content for peer review to be conducted inadequately.

Finally, a traditional protection against one type of failing is not as strong as one might hope, and recent developments have made it even weaker than it used to be. The failing in question is failure to be sufficiently critical of work by prestigious researchers. The protection, which is in any case not always used, is that the authors of papers are not identified to reviewers.

This protection has never been very strong because many areas of work are so specialized that it is easy for a reviewer to tell who has written a paper. And the protection has been weakened by the modern tendency for authors to release drafts, with the authors' names on them, in advance of submission for formal publication. Researchers who are competent to review papers will have read many of the drafts in their field, so if they are asked to review a paper with the author's name hidden, they may very well know immediately the identity of the author. One remedy is for reviewers who identify authors to declare when they have connections with those authors that might influence their judgement, such as their having had close working relationships with the authors in the past. But we cannot be confident that reviewers will always do so.

We have set out some reasons to be concerned as to whether traditional peer review is as good a control of quality as might be desired. But there are remedies. One remedy is to publish reviewers' reports and authors' responses, so as to increase the likelihood that the relevant community of researchers will notice if there is anything amiss. This approach has for example been implemented by *Biology Direct*.[44] Another remedy is the open comment that we shall

[44] http://www.biologydirect.com/.

discuss next, where the limitations of individual reviewers are counterbalanced by the fact that comments may be made by many people.

9.4.3 Open comment

It is now very easy to make work available online, whether in draft or in its final form, and to invite comment on it. The invitation may be limited to a select group of researchers, or it may extend to anyone who may be interested. The boundary between the periods before and after publication, and the boundary between official and unofficial comments, boundaries which used to be perfectly clear, may both become blurred. We shall give the name "open comment" to all such new ways of working, regardless of whether a select group or the public at large is invited to comment.

Infrastructure that facilitates open comment is now well-established in several disciplines. Repositories for preprints of papers include the *arXiv* for mathematics and certain natural sciences, and the *Social Science Research Network*.[45] These sites give contact details for authors, so people can write directly to authors rather than making comments public. Another option is to publish work and then publish comments alongside the papers. An example is *MediaCommons Press*. It offers this facility for documents in media studies, ranging from short reports to books, and uses software that allows comments to be linked directly to specific paragraphs of uploaded texts.[46] There are also systems for recording comments on papers that have been published or placed in online repositories

[45] http://arxiv.org/; http://ssrn.com/.

[46] http://mcpress.media-commons.org/. *MediaCommons Press* is not a journal in the traditional sense of an entity that publishes a steady stream of papers. Instead, it publishes documents sporadically.

elsewhere. Examples are *Naboj Dynamical Peer Review* and *PubPeer*, although some reviews on such sites are one-line comments rather than worthwhile analyses of the papers reviewed.[47] Another tool is *SelectedPapers.net*, which works with existing social networks.[48]

The spread of this way of working, involving more people and linking comments to the work commented on, should increase the probability that when a claim is not well-supported, someone will notice and will leave a record of the problem in a place where others are likely to find it. This may in turn deter attempts to publish claims that are not well-supported.[49] Open comment can also allow work to be appraised very quickly and efficiently, as with the appraisal of Vinay Deolalikar's putative proof that the complexity class P did not equal the class NP.[50]

Having said that, comments will only give rise to an effective control if they are found. Not all comments will be conveniently linked to the work in question. Anyone who wants to rely on claims made in order to make further progress should seek out comments on those claims, even if they were made in blog posts or in other places that are not covered by citation indices. This concern is likely to grow with the increasing academic use of blogs and other social media.[51] Fortunately, search engines give a reasonable prospect that comments will be found.

[47] http://www.naboj.com/; http://pubpeer.com/.

[48] http://docs.selectedpapers.net/.

[49] Koop and Pöschl, "Systems: An Open, Two-Stage Peer-Review Journal".

[50] Rehmeyer, "Crowdsourcing Peer Review".

[51] Krugman, "The Facebooking of Economics"; Nentwich and König, "Academia Goes Facebook? The Potential of Social Network Sites in the Scholarly Realm"; Puschmann, "(Micro)Blogging Science? Notes on Potentials and Constraints of New Forms of Scholarly Communication".

We have noted that traditional peer review rations publication. It need not do so, but rationing is an unsurprising consequence of the fact that resources are limited, combined with the fact that review precedes publication. Fortunately, new models of publishing that fit well with open comment also minimize rationing. There are repositories and journals that publish before review. There is also a variant in which papers are only published after review, but without any rule that papers must be particularly important. This variant is exemplified by the journals *PLOS ONE* and *Scientific Reports*.[52] Developments that reduce rationing may help to get reports of null results published. They are not however guaranteed to do so, nor to ensure that null results are presented in an unvarnished fashion. There is evidence that the motives of some researchers play a part in some non-publication or misleading publication.[53]

9.4.4 Repeated work

The ultimate form of independent appraisal of other researchers' work is to do the work again and see whether the results are the same. This is a costly exercise, so it is only performed for a modest proportion of pieces of work. We may have considerable confidence in claims that have been substantiated in this way, so long as we can be sure both that the work was done in the same way by independent researchers, and that all attempts to replicate a given result were registered in advance. Advance registration counters the risk of several attempts at replication being made, and only the successful ones being

[52] Guédon, "Sustaining the 'Great Conversation': The Future of Scholarly and Scientific Journals", pages 103-104.

[53] Couzin-Frankel, "The Power of Negative Thinking".

reported.[54] But interpretation of the results of attempts to replicate work can be a complex business. Success does not prove that claims made on the strength of earlier work were correct, nor does failure prove that they were incorrect.[55]

9.5 New technologies

New technologies have made a great difference to the conduct of research. These technologies can be classified under two broad headings, with a grey area at the boundary.

The first class comprises technologies that make it easier for researchers to manage information and find what they need, and to work in groups that are large, geographically dispersed or open to new members without formal processes of recruitment. Technologies in this class have their uses in many disciplines.

The second class comprises technologies that have given researchers new ways to acquire and analyse data. These technologies include sensitive detectors of particles, telescopes with great reach, tools to measure activity in human brains, tools to analyse the composition of historical artefacts, and techniques which make available the computational power that is needed to analyse large bodies of data or carry out demanding computations. Technologies in this class tend to be specific to particular disciplines or to a few disciplines. (One exception, applicable to many disciplines, is the provision of computational power.)

[54] For a discussion of the value of attempted replication and of the need for controls see Nosek and Lakens, "Registered Reports: A Method to Increase the Credibility of Published Results".

[55] Open Science Collaboration, "Estimating the Reproducibility of Psychological Science", Discussion section.

We shall limit our attention to the effects of technologies in the first class. Technologies in the second class are enormously important, but most of them are highly specialized. We shall also limit ourselves to mentioning some leading technologies that are already well-established. New technologies are developing rapidly, and there are several institutes at work in the area.[56]

9.5.1 The management of information

In many disciplines, there has been a huge expansion of the literature in recent decades. Researchers cannot hope to read everything that is published in their fields unless their fields are defined very narrowly indeed. And those who only read the literature in their own narrow fields may miss relevant information from other fields. They might then over-estimate the levels of support that claims enjoyed, because some of the missed material might have cast doubt on some claims or on the value of support of certain types.

Fortunately, help is at hand. Citation indices help researchers to track down comments on published work, so that they can find both objections to claims made and responses to those objections.[57] There are also computerized tools to conduct searches that are not limited to citations, from general-purpose web search engines to specialized tools that

[56] Examples are the Oxford e-Research Centre, http://www.oerc.ox.ac.uk/; the Oxford e-Social Science Project, http://microsites.oii.ox.ac.uk/oess/; the King's College London Centre for e-Research, http://www.kcl.ac.uk/innovation/groups/cerch/index.aspx; the University of Auckland Centre for eResearch, http://www.eresearch.auckland.ac.nz/en/centre-for-eresearch.html; the Maryland Institute for Technology in the Humanities, http://mith.umd.edu/.

[57] Several citation indices covering disciplines in the natural sciences, the social sciences and the humanities are brought together at *Web of Science*, http://wokinfo.com/.

are tailored to the needs of particular disciplines and that use sophisticated techniques to produce the most useful results.[58] Such aids reduce the risk of wrongly assenting to claims because of a failure to find relevant literature. A further development is the standardization of terminology in specific fields, so as to make it easier to unify databases and to find and share information. An example is *Gene Ontology*, which seeks to standardize the terminology used to describe gene products.[59]

Another development is the growth of specialized wikis, written and edited by many contributors. Pages in a wiki cover specific topics or methods. A page may set out claims, summarize other points that are made in the literature, and give links both to other wiki pages and to the literature.[60]

Wikis are undoubtedly useful. They can both supply information directly and help researchers to navigate the literature. But we must ask whether there is enough control over what gets posted on them to ensure that claims that are not well-supported, and recommendations to use methods that are not in fact reliable, will be eliminated quickly. The concern arises not merely in connection with specific pieces of information, errors in which may easily be corrected one by one, but also in connection with the overall views of topics that are presented. It has been argued that wikis can be a very useful way to bring

[58] An example of a specialized search tool is *GoPubMed*, http://www.gopubmed.org/web/gopubmed/. *GoPubMed* uses background knowledge of concepts and of how they are linked in order to search more intelligently than would otherwise be possible: http://help.gopubmed.com/.

[59] http://geneontology.org/.

[60] Examples of specialized wikis are the *nLab*, http://ncatlab.org/, in mathematics; *OpenWetWare*, http://openwetware.org/, in biology; *Glottopedia*, http://www.glottopedia.org/, in linguistics.

together different perspectives on topics.[61] But openness to different perspectives naturally raises concerns, both about the appropriateness of the perspectives and about the correctness of specific claims that are made from those perspectives.

The standard control in wikis is one that does not at first sight look like much of a control. It is that anyone, or anyone drawn from a large group, can edit entries. Anyone who thinks that material is incorrect can change it. If a claim is made with no reference to support, anyone can add “Citation needed”. The record of all changes is often public, and there is often a discussion area for each page where contentious issues can be debated. So long as enough researchers are interested in improving a specialized wiki, and so long as all participants act sensibly, not pursuing editing wars in which the same changes are made back and forth several times, we may reasonably hope that most of the contents of the wiki at any given time will be academically respectable.

This is however only a hope. Studies of reliability have been done, but they have concentrated on *Wikipedia* rather than on specialized wikis.[62] An assessment of specialized wikis would be a substantial task. Disciplines and their cultures are so diverse that it would be necessary to test wikis in specific disciplines, rather than testing a few wikis and basing a general conclusion as to reliability across disciplines on that sample. Moreover, systematic tests of

[61] Bauer, “Multivocality and ‘Wikiality’: The Epistemology and Ethics of a Pragmatic Archaeology”.

[62] For an example of an assessment of *Wikipedia* on a topic that would mostly attract the attention of specialists see Halsted, “Accuracy and Quality in Historical Representation: Wikipedia, Textbooks and the Investiture Controversy”. This study found some inaccuracies in the relevant *Wikipedia* article, but also praised the article for completeness.

any given wiki would be needed to give a high level of reassurance. It would not be enough merely to check that there were plenty of active expert readers who could detect and correct errors. That would not be enough because the normal indicators that a reader can use to assess the reliability of material, such as the authority of authors, whether material is written in the style that is typical of experts, and the correctness of other material on topics about which the reader is well-informed, may not be as effective in relation to wiki articles as they would be in relation to more traditional material.[63] There is however evidence to support the reassuring claim that academic users of the world wide web assess the quality of the information that is presented to them.[64]

There are other controls. Some wikis limit editing rights to people who have been approved.[65] There are also sites that move some distance away from the standard wiki model by peer-reviewing articles and amendments to them before publication.[66]

There is also a control that does not bear directly on the contents of wikis but that limits the risk that inadequacies in those contents will come to infect the body of scholarly literature, and will hence lead to claims being regarded as better supported than they really are. This control is that it is not generally considered acceptable to cite a wiki article in support of a claim. It is perfectly acceptable

[63] Magnus, "On Trusting *Wikipedia*". Although this paper relates to *Wikipedia*, the concerns expressed arise to some extent in relation to specialized wikis.

[64] Rieh, "Judgment of Information Quality and Cognitive Authority in the Web". This study relates to the web generally, rather than to wikis in particular.

[65] *OpenWetWare* is an example: http://openwetware.org/wiki/OpenWetWare:How_to_join.

[66] *Scholarpedia* does this: http://www.scholarpedia.org/.

to find material by following up citations in wikis. If the cited material is checked at first hand, it can then be cited in support of new claims. But references to wiki articles themselves are not expected to appear in footnotes. One reason why wiki articles should not be cited is that material in them is generally derived from other published sources. Researchers should go back to those sources, and sometimes to sources on which they in turn were based, rather than relying on the summaries and interpretations of sources that appear in wiki articles. Another reason why wiki articles should not be cited is that their texts are sometimes subject to frequent change. Someone who cited a wiki article could specify the time at which she read it, but although it might be possible for readers of the citation to view the article as it stood at that time, that would be an extra task and one that might not always be feasible.

9.5.2 Large, dispersed and open groups

New technologies facilitate work in groups that may be large, geographically dispersed, or open to new members who can join without any formal process of recruitment. We shall count as members all people who contribute to a group's work. Some groups have formal processes of recruitment, some have members who meet certain membership criteria but who come and go as they please, and some throw membership open to the public (who will also come and go as they please).

We may identify three main types of contribution of new technologies to facilitating work in large, dispersed and open groups.

The first type of contribution is that the Internet and online storage make it easy to make material available around

the world. The material may be textual or numerical data, scanned copies of documents, or photographs of artefacts or natural phenomena.

The second type of contribution is that blogs and online fora can be used to invite people to contribute to the solution of problems. In one form of this type of work, contributions to separate aspects of a problem are invited. The definitions of different aspects of a problem may be quite loose at the start, and may change as people contribute their thoughts. Such a project may have a central manager who will periodically summarize progress to date and start a new thread of discussion which takes account of that progress. The leading example is *Polymath*, which was originated by Timothy Gowers and which has tackled several different mathematical problems.[67] In another form of this type of work a single problem, such as that of how to analyse large amounts of data efficiently, may be offered to the world at large, perhaps with some incentive to contribute ideas. The results can be impressive.[68]

The third type of contribution is this. When a project involves producing a collective statement that sets out the work done and the results, distributed version control systems such as *Git* make it easy for members of a group to add their contributions and their editorial changes to a developing statement without having to seek permission from other members, and to do so with confidence that all changes will be tracked, conflicting changes will largely be avoided, and any conflicts that do arise will be resolved.[69] A

[67] http://polymathprojects.org/; Gowers and Nielsen, "Massively Collaborative Mathematics".

[68] Lakhani et al., "Prize-Based Contests Can Provide Solutions to Computational Biology Problems".

[69] For information on *Git* see http://git-scm.com/. An example of its use is the writing of a book on homotopy type theory by a large group of mathematicians in a short space of time:

variant is to set up a wiki with appropriate initial articles and categories of articles, and then invite either selected researchers or the community at large to add information and edit articles. At some point the contents of the wiki will represent a collective statement of the work done and the results.[70]

Both version control systems and wikis can also be used to facilitate work by different people on different aspects of a problem. That is, these technologies can make contributions of the second type mentioned above.

In section 9.2.4, we discussed some of the risks that arise out of the scope for groups to impose orthodoxy. Work that would be productive but would not fit in with mainstream beliefs or ways of working may be met with hostility or neglected, diversity in debate may be reduced, and groupthink may arise. We may ask about the effects of groups being large, geographically dispersed, or open to new members who may join without any formal process of recruitment.

The risks should be reduced. The larger a group, the greater the range of points of view its members should have. The more geographically dispersed a group, the less likely it is that people will express agreement against their better judgement: it is less important to keep the peace with colleagues when one does not work in the same building. And openness to new members presents an opportunity for dissident voices to challenge orthodoxy.

Univalent Foundations Program, *Homotopy Type Theory: Univalent Foundations of Mathematics*. For the process of writing the book see Bauer, “The HoTT Book”.

[70] For examples of the use of wikis in research see Bauer, “Multivocality and ‘Wikiality’: The Epistemology and Ethics of a Pragmatic Archaeology”, pages 189-193.

One theme that runs through use of the technologies discussed here is that of openness. Openness to anyone who wishes to participate in a research project is not an inevitable accompaniment to use of these technologies, but their use does facilitate that openness. These technologies also facilitate a different type of openness, that of allowing the methods developed and the data obtained in one project to be used in other projects. Moreover, methods, data and results from a project can be organized in a single useful bundle, a Research Object.[71]

The growth of openness is having a significant impact on the conduct of some disciplines, in particular some of the natural sciences.[72] There is a view that, particularly in the natural sciences, the impact is enough to justify identifying and naming a whole new way of working. The term "Science 2.0" has come into use as a name for work that involves large amounts of data, the open exchange of interim results long before final conclusions are ready to be published, and large-scale collaboration.[73]

Openness in all of its forms should increase our confidence in accepted claims, both because work is exposed to a wide range of contributors and potential critics, and because researchers who can borrow data and methods from others can make faster progress with their own work, obtaining new results that may expose defects in previously accepted claims or in the support for them.

[71] For an example see Hettne et al.,"Structuring Research Methods and Data with the Research Object Model: Genomics Workflows as a Case Study".

[72] Bartling and Friesike (eds.), *Opening Science: The Evolving Guide on How the Internet is Changing Research, Collaboration and Scholarly Publishing*; Royal Society Science Policy Centre, *Science as an Open Enterprise: Final Report*.

[73] Bartling and Friesike, "Towards Another Scientific Revolution"; Gray, "Jim Gray on eScience: A Transformed Scientific Method".

9.6 Epistemic virtues

9.6.1 The need for epistemic virtues

If researchers exercise epistemic virtues, that will increase both their effectiveness and our confidence. In addition, it will help to make it safe for other researchers to rely on their work. It is essential that other researchers can do so with confidence, and this requires trust between researchers. Evidence of epistemic virtue can play an important role in supporting the necessary trust.

There is therefore ample reason to explore the epistemology of testimony and the role of trust in that epistemology. The connection has long been recognized in the philosophical literature.[74] But that is not the approach we shall take here. The task set by our main question is not to construct an epistemology, in the sense of a theory that would specify conditions for people to have knowledge. Instead we shall identify particular virtues, the exercise of which can give reassurance as to the correctness of claims.

Since our task is not to construct an epistemology, not even a virtue epistemology, we shall not be concerned with the distinction between virtue reliabilism and virtue responsibilism. We shall draw on both approaches. Our concern with what will in fact allow us to have confidence might seem to align us largely with reliabilism, but on the other hand virtues such as completeness, which requires not disregarding inconvenient data, can be seen as a character trait of the kind that interests responsibilists.

Our concern with what works means that we shall only consider a virtue to be exercised if it is exercised in a way

[74] Hardwig, "The Role of Trust in Knowledge", section 1.

that has a good prospect of being effective. Good intentions are not enough. Having said that, we shall regard a virtue as exercised when the intention and the necessary skill are both present, even if some other circumstance leads a researcher to make or assent to an incorrect claim. We also do not deny the importance of values, which are simply held rather than exercised, as psychological supports for the possession of virtues.[75]

Finally, while it is natural to think of epistemic virtues as being exercised by individuals, they can also be exercised by groups of researchers. A group as a whole might be particularly careful about considering all relevant information, or it might be particularly imaginative, while the group's possession of such virtues might not appropriately be seen as merely comprising possession of the virtues by members of the group.[76] We shall not remark on this possibility at each stage, but each reference to the exercise of an epistemic virtue should be read as including its exercise by a group.

We shall now consider some virtues that are particularly important in relation to each of the making of claims, the use of imagination and the appraisal of claims, before concluding with a note on virtues and the integrity of research.

9.6.2 The making of claims

One essential virtue is accuracy. If researchers measure the values of variables, or carry out surveys, or record the texts

[75] For how values can be mapped, their legitimate roles in research, and the risks that arise when values influence the appraisal of claims inappropriately, see Thagard, *The Cognitive Science of Science: Explanation, Discovery, and Conceptual Change*, chapter 17.

[76] Lahroodi, "Collective Epistemic Virtues".

or the physical features of manuscripts, they must note down what they find, and only what they find.

A related virtue is completeness. If researchers take several measurements they must note down all of them, and not ignore or regard as unimportant ones they did not expect or that do not support their pet theories. If a measurement is strange, researchers may mark it for further investigation in case it resulted from an error, but they should not simply ignore it or decide without good reason that it should not be given its full weight. The importance of giving due weight to measurements that do not support a theory is illustrated by a study that was claimed to demonstrate the so-called memory of water. The team regarded positive results more favourably than negative results, and this was one of the concerns that others had about their work.[77]

In some disciplines, there are systematic approaches to the collection of evidence. For example, there are systems to use when recording archaeological excavations, and systematic approaches to the conduct and recording of interviews in the social sciences.[78] The existence of such detailed guidance can make it relatively easy to demonstrate that the virtue of completeness has been exercised. But where detailed guidance is not available, researchers must use their judgement as to what should be recorded and what should be given weight. It may then be less clear whether work done is complete, or indeed whether there is a well-defined standard of completeness to be met.

If either of the two virtues of accuracy and completeness is not exercised, claims that are not well-supported can all

[77] Maddox, Randi and Stewart, "'High-Dilution' Experiments a Delusion", page 290.

[78] Museum of London Archaeology Service, *Archaeological Site Manual*; Bryman, *Social Research Methods*, chapter 20.

too easily be made. We must therefore be concerned as to whether failures to exercise these virtues are generally detected. It is not just a matter of detecting deliberate failure, although there are cases of that.[79] Cases in which the intention to be virtuous is present, but the skill that is necessary to make the intention effective is lacking, also give rise to a serious risk that claims will be made when they should not be made.

We cannot measure the extent of failures to exercise the virtues of accuracy and completeness with any precision. We can gain some reassurance from the fact that researchers are a critical breed, and are likely to ask questions about any startling claims. On the other hand, if a claim is humdrum, and if it raises no questions about established results, inadequacies in work done to support it may go unnoticed. We may also be concerned that limits on resources, sociological factors, and varying understandings of what amounts to misconduct may all lead to inadequacies not coming to the notice of the research community.[80] If inadequacies can go unnoticed, that will to some extent disable the control of demanding adequate support for claims before assenting to them. Then, given that some incorrect claims will be made, some incorrect claims will come to be accepted.

Moving on to arguments that take readers from the evidence that is reported to the claims for which a publication argues, the links from evidence to claims need to be strong, either individually or collectively. Researchers need to exercise the virtue of sound reasoning. It might be thought that

[79] Martinson, Anderson and de Vries, “Scientists Behaving Badly”; Fanelli, “How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data”.

[80] Hardwig, “The Role of Trust in Knowledge”, section 3; Fanelli, “How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data”, Discussion section.

others would routinely check whether reasoning was sound by working through published arguments. But researchers may well lack the time needed to check all the steps in arguments. Moreover, the acceptability of steps in a chain of reasoning may be a matter of judgement. And when there is scope to exercise judgement, skilful presentation can make some steps more appealing to the reader than they would otherwise be.[81] It does not however follow that rhetoric should be excluded from the presentation of research. In some disciplines, it plays too central a role to be eliminated.[82]

There is an over-arching virtue, the exercise of which would require all three of accuracy, completeness and sound reasoning. This is the virtue of only making claims when one both has enough evidence and understands why the evidence suffices. The most forthright injunction to exercise this virtue was issued by William Kingdon Clifford, in the words, "To sum up: it is wrong always, everywhere, and for any one, to believe anything upon insufficient evidence".[83] The adoption of an attitude as severe as Clifford's would reduce the number of claims that were put forward without adequate support. Given that the support for claims is not always fully checked, that would in itself reduce the number of claims that came to be accepted without their having adequate support. And where checks were made, the adoption of Clifford's attitude would help because it would mean demanding to see the support for claims, a demand that can often expose incorrect claims. But Clifford's attitude could also impede progress, because it could make researchers too reluctant to make claims.

81 Barahona and Cachón, "The Rhetorical Dimension of Stephen Jay Gould's Work".

82 Its pervasive role in economics is set out in McCloskey, *The Rhetoric of Economics*.

83 Clifford, "The Ethics of Belief", section 1, page 175.

This is an area in which researchers must exercise judgement, guided by the norms of their discipline. It would be pointless for researchers to take Clifford's attitude to such an extreme that their disciplines could not be advanced, but unacceptable to violate Clifford's injunction without making it clear what had been done. A convention in some disciplines is to provide a reference whenever a piece of information is directly substantiated by a source, so that the absence of a reference indicates that there is no direct support, and to use phrases like "We may assume that" and "He must have appreciated that", to signal any claim that is a reasonable supposition rather than being based directly on evidence.

A related concern arises out of the use of reasoning that is far from deductive. If the path from evidence to a claim would not force anyone who acknowledged the evidence to assent to the claim, it might be thought that assent to the claim would violate Clifford's injunction, at least under a strict reading of the requirement for sufficient evidence. Researchers must again exercise judgement. Some non-deductive steps may be perfectly acceptable so long as their non-deductive nature is obvious to the reader, but other steps may be unacceptable.

The acceptance of reasonable suppositions that are clearly signalled, and of reasoning that is far from deductive, should not however be allowed to develop into permission to make suppositions or to reason loosely in ways that are guided by a desire to reach conclusions which have been selected in advance. That practice is always an epistemic vice.

9.6.3 The use of imagination

Researchers' use of imagination must display two virtues if it is to reduce the risk that incorrect claims will come to be accepted. Imagination must be lively, and it must also be disciplined. We can see the roles that these virtues play by considering the course of work on a problem, from initial ideas to the testing of conclusions.

When researchers seek possible answers to their questions, or possible new questions, or possible ways to appraise claims, it is helpful to give imagination free rein. There is no need to hold back for fear of error, because researchers will subsequently reflect on the fruits of their imagining. Having said that, imagination can usefully be guided. In experimental disciplines, the guidance provided by recognized ways to design and conduct experiments can be vital, opening up possibilities while limiting the scope to stray down byways that are unlikely to be productive.[84] And any period of speculation must be followed by a period of sober reflection. This need for sober reflection is not limited to the consideration of claims that might be made. It also extends to the consideration of work that might be done to answer questions or to appraise claims that have already been made. Researchers should direct their limited resources intelligently, not pursuing avenues of exploration that might in principle be useful but only at disproportionate cost. This virtue is identified by Adam Morton as "experiment-shopping".[85]

[84] The notion of a recognized way to design and conduct experiments can be broadened to the notion of an experimental system, a whole context of work to which Hans-Jörg Rheinberger gives a central role: Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, pages 19-21 and 24-28.

[85] Morton, "Acting to Know: A Virtue of Experimentation", section 4.

When researchers have identified candidate claims that are worth taking seriously and move on to appraise the candidates, imagination is again needed, but it must be very disciplined. Researchers might for example need to test a claim that the unobserved occurrence of some phenomenon C explained the observed occurrence of some other phenomenon E, when there might for all they knew have been an unobserved occurrence of D instead of C which might equally well have led to E. Disciplined imagination would be needed in order to identify all the likely observable consequences of C and of D. It would also be needed in order to appraise the plausibility of situations in which C might occur without E's occurring.[86] Researchers also need disciplined imagination to guide their search for any evidence that might conflict with claims. They must think of the right places in which to look, or the right experiments to conduct.

In some parts of the natural sciences, tests of some types follow established patterns. A new drug, for example, must be tested using a randomized controlled trial, with samples that are large enough to allow statistical conclusions to be drawn. But researchers need to be aware of reasons why standard tests might be insufficient. Lively imagination should help researchers to think of circumstances in which additional tests would be needed.

In disciplines that are relatively high up the scale, and particularly in the humanities, there are often not enough standard tests to use. It may even be inappropriate to think in terms of all tests being devised in advance

[86] For the workings of the imagination in relevant types of reasoning, albeit with examples that are mostly drawn from everyday life rather than from academic work, see Byrne, *The Rational Imagination: How People Create Alternatives to Reality*, chapters 2, 5 and 6. For a range of roles of imagination and of thought experiments see De Mey, "Imagination's Grip on Science".

and then applied. Rather, the process of appraisal may involve inventing tests after the event, making connections with other information in order to assess arguments, and sometimes coming up with alternative interpretations of the evidence. Whether or not tests devised in advance predominate, a lively imagination is essential in order to have a good prospect of weeding out incorrect claims.

The initial search for possible claims and consideration of their prima facie plausibility might be regarded as falling within the context of discovery, while the appraisal of claims might be regarded as falling within the context of justification. But that distinction between contexts is contentious. Even if it is accepted, it is more complex than it might at first seem to be.[87] And the proper use of imagination at an early stage can contribute to the justification of claims. We shall consider two ways in which it can contribute. The first one relates to the fallibility of processes for appraising claims, and the second one relates to the desirability of considering a wide range of candidate explanations of phenomena.

A consequence of the fallibility of appraisal processes is that we should attach significance to how claims come to be made. We should do so because if claims are made following sound reasoning, that should increase the proportion of claims that are correct. That should in turn reduce the extent to which deficiencies in appraisal would lead to the acceptance of incorrect claims, given that some proportion

[87] Schickore and Steinle (eds.), *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*, particularly chapter 2: Howard, "Lost Wanderers in the Forest of Knowledge: Some Thoughts on the Discovery-Justification Distinction"; chapter 10: Nickles, "Heuristic Appraisal: Context of Discovery or Justification?"; chapter 13: Arabatzis, "On the Inextricability of the Context of Discovery and the Context of Justification".

of incorrect claims will slip through the process of appraisal.

In particular, the imaginative steps that are taken to move from evidence to claims can be significant. If those steps are the work of researchers with disciplined imaginations, that should reduce the proportion of incorrect claims. A disciplined imagination is one that keeps in mind the existing corpus and the types of reasoning that have been found to be successful in the past, and that does not drift off into vague musings.[88] If on the other hand claims are reached by taking imaginative steps that amount to undisciplined jumping to conclusions, that would be likely to increase the proportion of incorrect claims. That would in turn put pressure on the process of appraisal, having an adverse effect on our confidence.

Turning to ranges of candidate explanations, we discussed the desirability of researchers considering an appropriate range of candidates in section 8.3.3.1. If researchers tackling a given question have lively imaginations that they use appropriately, they will think of a wide range of candidates. That can in turn lend support to their decisions to endorse certain explanations. It can do this because it can lessen the risk that the candidates found will appear to be of high quality merely because not enough candidates have been considered. (Remember that an explanation that is satisfactory in itself may be suspected of missing the point if other explanations are much better.) This can in turn lend support to the associated claims to explain, and to claims that play explanatory roles. The use of imagination to identify an appropriate range of candidates is particularly important when claims within explanantia

[88] Timothy Williamson has noted the importance of disciplining the imagination by insisting on rigorous and precise thought: Williamson, *The Philosophy of Philosophy*, page 289. He writes in relation to work in analytic philosophy, but the point extends to other disciplines.

are to be supported, because of the condition we introduced in section 5.1.2.2 that the relevant explanations should be better than others.

9.6.4 The appraisal of claims

We shall now discuss other epistemic virtues in the context of the appraisal of claims, virtues that researchers should exercise if they are to make sound judgements as to whether to assent to claims.

It is important for researchers to be free of certain prejudices. They should not be influenced by their personal views as to the attractiveness or unattractiveness of certain ways of working (as opposed to being influenced by well-founded judgements that certain methods are reliable while others are unreliable). Nor should they be influenced by views as to the abilities of other researchers which do not reflect those researchers' true merits.

Inappropriate views on individual researchers can obviously affect the appraisal of specific pieces of work. This risk is a major reason why there is a tradition that peer reviewers should not know the authors of papers – a protection that is of limited effectiveness, as we noted in section 9.4.2. There may also be systematic biases against whole classes of people or in favour of other classes. The main effect of such systematic biases may well be at the stage of allocating resources to particular lines of work rather than at the stage of appraising results.[89] But if resources are allocated inappropriately, the damage may extend beyond a failure to do potentially valuable new work. The scope to use new

[89] Wray, "Evaluating Scientists: Examining the Effects of Sexism and Nepotism".

work to challenge other claims may be lost too, so that claims may survive when they should be discarded.

It can be difficult to detect prejudices as to ways of working. A personal opinion that is not widely shared should certainly be regarded with suspicion. But there may also be prejudices that are not recognized as such, simply because they have taken hold over the years and are very widely shared. These prejudices may lead researchers to reject claims out of hand because they arise out of work done in non-standard ways, when the claims should in fact be given serious consideration and should sometimes come to be accepted. Indeed, novel approaches can themselves be a source of considerable progress. Researchers who adopt particular ethical or political positions, such as a commitment to certain feminist values, or who are for any other reason sensitive to long-standing biases in the approach to objects of study, may be well-placed to advance their disciplines by rejecting traditional assumptions and looking at the evidence afresh.[90]

It is therefore important for those who appraise the work of others to be alert to the possibility that their own assumptions might lead them to make mistakes. But this does not mean that standards should be relaxed. The fruits of novel approaches must be subjected to rigorous scrutiny, just like the fruits of any other research. It is also important to restrict the roles that ethical and social values of any sort may play, and in particular to prevent inappropriate influence on the design of studies or on decisions as to whether to make or assent to claims.[91] (We here comment specifically on the roles of values that are fundamentally

[90] There are examples in Kourany, "Replacing the Ideal of Value-Free Science"; Wylie and Nelson, "Coming to Terms with the Values of Science: Insights from Feminist Science Studies Scholarship", section 3.2.

[91] Douglas, *Science, Policy, and the Value-Free Ideal*, chapter 5.

ethical or social. The case for imposing restrictions does not extend to values that are fundamentally epistemic, such as a commitment to observe norms of evidence and argument, nor to norms of evidence and argument themselves, for example a norm that a null hypothesis is only to be rejected on the basis of $p < 0.05$, or a norm that simple theories are to be investigated to see whether they are satisfactory before resorting to complex theories.[92] We also do not here consider personal goals, such as the goal of advancing one's career. We shall touch on personal goals in section 9.6.5.)

There are also some entrenched preferences that play valuable roles in making disciplines productive. An example would be a preference for working in ways that have been found regularly to lead to conclusions which have then been substantiated in other ways. Another example would be a preference for allocating resources to research that was defined in detail by reference to the existing corpus, whether as building on that corpus or in opposition to it. The unfunded heretic who ignores the corpus, and who then rails against the prejudice of the establishment, may fully deserve his neglect. It is therefore not right to deprecate any preference that looks as though it might be a prejudice. We can only ask that researchers and those who allocate resources should be aware of their preferences and of the risk of baleful effects.

Another virtue to exercise is that of sound judgement in the choice of standards and methods of appraisal. In large parts of the natural sciences, judgement may be supported by detailed guidance. The required types of test of claims, and the levels of statistical significance that must be displayed

There is however scope to argue that Douglas is still too generous in allowing values to have influence.

[92] For the scope to separate social and epistemic values see Doppelt, "Values in Science".

in order for those tests to be passed, are fixed by the norms of disciplines. Higher up the scale of disciplines, there is less guidance on methods to use. Moreover, the available methods of appraisal are often not tests that make use of notions of statistical significance. Judgement must therefore be exercised when determining whether claims meet satisfactory standards.

The exercise of judgement in choosing methods of appraisal and in deciding whether claims meet satisfactory standards does not amount to a relaxation of standards in some disciplines, or when appraising some claims. Disciplines are different, and claims made within them need to be appraised in different ways. We should not see standards that are commonly applied in the social sciences or the humanities as relaxed versions of standards that are applied in the natural sciences, but as different standards. (There is however some overlap, as when tests of statistical significance are used in the social sciences or the humanities.)

This observation does not deny natural scientists the opportunity to say that their accepted claims have been appraised more rigorously than accepted claims in the social sciences or the humanities, and hence to say that their accepted claims enjoy stronger support. But the reason why there is less support for accepted claims in the social sciences and the humanities is not that those claims can only pass the tests that are used in the natural sciences when the bar is lowered. The reason is that those tests often cannot be used. Moreover, these reflections give us no reason to think that researchers cannot tell when they should or should not assent to claims in the social sciences or the humanities.

Neither the tailoring of tests to disciplines nor the exercise of judgement in the appraisal of claims should be seen

as amounting to the invocation of contextualism, subject-sensitive invariantism or any comparable position that introduces more flexibility into attributions of knowledge than has been traditional in epistemology.[93] Such novel epistemological views have been developed in order to analyse the concept of knowledge, but there would be an easy extension to the question of whether researchers should assent to claims. One might for example say that whether researchers should assent to a claim should depend on contextual features such as whether the claim would play an important role, perhaps in providing support for other claims or perhaps in organizing the corpus, so that the stakes were high. (We run together a variety of epistemological views. That would not be appropriate if we were concerned with the conditions for attributing knowledge. The positions differ in ways that are important in that context. But in our context, the thing that matters is the degree of flexibility that is introduced, where flexibility reflects the attachment of importance to non-traditional factors.)

There is good reason to distinguish both the tailoring of tests and the exercise of judgement in the appraisal of claims from the invocation of such novel epistemological views. In academic disciplines the task is to decide whether to assent to claims, primarily for the sake of enlarging the corpus rather than with a view to any practical application of claims. There should therefore be less temptation than in practical life to allow standards to be influenced by factors that are made relevant by the needs and interests of the people involved, such as the level of the stakes. In academic disciplines, needs and interests should not vary to anything like the extent that they vary in practical

[93] For a map of some of these positions see Greco, *Achieving Knowledge: A Virtue-Theoretic Account of Epistemic Normativity*, chapter 7, part 2, section 2.

life. The interest should be a stable one, in assenting to claims that are correct and rejecting claims that are incorrect. It is noteworthy that arguments which make room for contextualism, and which make its appeal perfectly comprehensible, focus on needs and interests. In so doing they put in question any view that contextualism ought to apply in the context of academic disciplines.[94]

Having argued against the relevance of such novel epistemological views to academic disciplines, we should acknowledge that an important claim with wide implications probably will receive more scrutiny than a claim that will not play any central role. That does not however mean that researchers should assent to minor claims more readily than major claims. We should also acknowledge that desires for prestige and funding may tempt researchers to relax their standards in order to make claims that are significant or

[94] One example of a focus on needs and interests is Hannon, "The Practical Origins of Epistemic Contextualism". Hannon has a broad conception of relevant factors, covering not only the immediate interests of someone who may or may not have knowledge but also facts about the relevant epistemic community (pages 912-913). This might be expected to make Hannon's argument easy to extend to academic disciplines, where the needs of the community take priority over those of the individual. But this cannot overcome the difficulty that needs and interests do not vary in academic disciplines to the extent that they vary in practical life.

Another paper, which discusses pragmatic encroachment in general, is Fantl and McGrath, "Pragmatic Encroachment: It's Not Just About Knowledge". At first the paper seems to be about to make a case for pragmatic encroachment on warrant for belief which is independent of a context of practical action. Positions called fallibilism and purism are both formulated without reference to action (page 29; purism is opposed to pragmatic encroachment). But the authors go on to argue their case by reference to situations in which the rationality of action is central.

We should however note that possible applications to academic disciplines are not discussed in either paper, so no views on that point should be attributed to the authors.

that can justify doing further work. But what does happen may not be what should happen.

Contextualism as one of the novel epistemological views that are in question here is to be distinguished from contextualism in the sense of the view that the significance of evidence depends on background assumptions. That sort of contextualism has considerable relevance to the picture painted in this work. It is also easily accommodated in that picture, in the form of recognition that the existing corpus is essential to the conduct of new research.[95]

9.6.5 The integrity of research

Communities of researchers in many disciplines are under pressure to secure shares of limited resources. There can also be commercial interests, both in the publication or non-publication of certain results and in the promotion of particular types of work that may lead to commercially viable products. In such an environment, there are constant threats to the integrity of the overall process of research.

Commercial interests give rise to obvious risks.[96] But there are also some less obvious risks, and any damage done may not be immediately apparent. For example, professional esteem may be allocated on grounds other than merit, and this can happen without anyone's consciously engaging in manifestly unethical behaviour.[97] To take another example,

[95] This relevant form of contextualism is discussed in Longino, "What's So Great about an Objective Concept of Evidence?".

[96] Brown, "The Community of Science®"; Adam, "Promoting Disinterestedness or Making Use of Bias? Interests and Moral Obligation in Commercialized Research", pages 242-243 and 246.

[97] Brennan and Pettit, "The Hidden Economy of Esteem", sections 4 to 6.

publishers of journals may encourage authors to include irrelevant citations in their papers so as to improve the impact factors of the journals that published the works cited, and this may unjustifiably improve the citation records of the authors cited.[98]

The virtues of individual researchers are one line of defence, alongside the constant threat of public criticism of distorted work. And there is a virtue that is specific to this context, the virtue of keeping to the rules, such as rules that require the disclosure of interests or that require refusal to work under conditions that would limit access to data or the ability to analyse data or publish conclusions.[99] But while the virtues of individuals are important as a line of defence, their direct relevance to our main question is very largely covered by what we have already said. We covered virtues as a defence against the falsification or the selective reporting of results in section 9.6.2. The misallocation of resources and the over-valuation of work have a more indirect bearing on our confidence in the claims that emerge from whatever work is done. If misallocation and over-valuation become serious, work that should be done in order to test claims which are already accepted, perhaps using methods that were not available when the claims first came to be accepted, may not get done.

There is also a very general concern, indirectly relevant to our main question, that if there is any slippage in any standard, researchers may cease to see the point of rigorous adherence to high standards. Then standards that mattered directly to our confidence might come to be ignored. But such a general concern should not lead to a demand that

[98] Wilhite and Fong, "Coercive Citation in Academic Publishing".

[99] One set of rules is International Committee of Medical Journal Editors, *Recommendations for the Conduct, Reporting, Editing, and Publication of Scholarly Work in Medical Journals*.

all researchers should be dedicated solely to the progress of their disciplines and should lack personal ambition. The desire to receive credit for work done and hence to be esteemed by one's colleagues need not obstruct progress.[100] There are even potential benefits of researchers' being guided by personal goals, such as a desire for professional success when they choose which projects to pursue or a desire for wealth when they work on the application of the results of research.[101] Individuals matter, as well as research groups and communities.

[100] Goldman and Shaked, "An Economic Model of Scientific Activity and Truth Acquisition".

[101] Thagard, *The Cognitive Science of Science: Explanation, Discovery, and Conceptual Change*, pages 291-292.

Bibliography

No editions are specified for classic works that are available in many editions, unless references in the text have been given in forms that can only be used with specific editions.

Some links to websites have been provided. We have preferred to give links based on DOIs where possible. Where an entry is given for a chapter within a book, and a link is available for that chapter, the link is to the chapter (although it is to the book if no link is available for the chapter). If there is a link for the book, it is given in the separate entry for the book as a whole. Links may of course cease to work. This can even happen with DOIs, for example when a journal and its online archive move from one publisher to another and the locations to which DOIs point are not updated.

The terminology in references to journal papers has been standardized as "volume" and "number", and Roman numerals have been converted to Arabic numerals. Volume, number and year are given. Months of publication sometimes vary as between online and printed editions, and are sometimes not given at all. They are only given here when they would be of material assistance in finding papers.

There has been some standardization of capitalization, taking account of the conventions of the relevant language. English-style quotation marks that fall within the double quotation marks that surround the titles of papers have been standardized as single quotation marks.

For the purposes of alphabetical order, the space is treated as the first character, and the hyphen as the second character. Characters with diacritics are treated as if they did not have them. Thus "ö" is treated as "o", not as "oe". Apostrophes are treated as if they were not there, so that "O'Hear" is treated as if it were "Ohear". "Mac" and "Mc" are treated according to their actual spelling, and not all as "Mac" or all as "Mc". "St." is placed after "Ss". An initial "The" or "A" is ignored for the purposes of alphabetical order when it occurs in the name of an institution as author, or in the title of a journal, but not when it occurs in any other title. Works by a single author are placed before works by that author and others. When a work in a language other than English has been cited in its original language and a translation is also listed here, the translation is listed after the original.

Abbott, Derek. "The Reasonable Ineffectiveness of Mathematics". *Proceedings of the IEEE*, volume 101, number 10, 2013, pages 2147-2153.
http://dx.doi.org/10.1109/JPROC.2013.2274907

Aberdein, Andrew, and Ian J. Dove (eds.). *The Argument of Mathematics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-6534-4

Abrams, David S. *Estimating the Deterrent Effect of Incarceration Using Sentencing Enhancements*. University of Pennsylvania Law School Institute for Law and Economics Research Paper number 11-13. Philadelphia, PA, University of Pennsylvania, 2011.
http://dx.doi.org/10.2139/ssrn.1819503

Accountability in Research: Policies and Quality Assurance. Journal. ISSN 0898-9621, eISSN 1545-5815.
http://www.tandfonline.com/loi/gacr20#.VUs8wzc2tyQ

Acerbi, Alberto, Vasileios Lampos, Philip Garnett and R. Alexander Bentley. “The Expression of Emotions in 20th Century Books”. *PLOS ONE*, volume 8, number 3, 2013, e59030, pages 1-6.
http://dx.doi.org/10.1371/journal.pone.0059030

Achinstein, Peter. *Evidence, Explanation, and Realism: Essays in the Philosophy of Science*. Oxford, Oxford University Press, 2010.

Achinstein, Peter (ed.). *Scientific Evidence: Philosophical Theories and Applications*. Baltimore, MD, Johns Hopkins University Press, 2005.

Adam, Matthias. “Promoting Disinterestedness or Making Use of Bias? Interests and Moral Obligation in Commercialized Research”. Chapter 11 of Martin Carrier, Don Howard and Janet A. Kourany (eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*. Pittsburgh, PA, University of Pittsburgh Press, 2008.

Aghion, Philippe, and Peter Howitt. *The Economics of Growth*. Cambridge, MA, MIT Press, 2009.

Akman, Varol, Paolo Bouquet, Richmond Thomason and Roger A. Young (eds.). *Modeling and Using Context: Third International and Interdisciplinary Conference, CONTEXT 2001*. Berlin, Springer, 2001.
http://dx.doi.org/10.1007/3-540-44607-9

Alama, Jesse, and Reinhard Kahle. "Checking Proofs". Chapter 9 of Andrew Aberdein and Ian J. Dove (eds.), *The Argument of Mathematics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-6534-4_9

Albert, Hans. *Traktat über kritische Vernunft*, 5., verbesserte und erweiterte Auflage. Tübingen, J. C. B. Mohr (Paul Siebeck), 1991.

Albert, Hans. *Treatise on Critical Reason*, translated by Mary Varney Rorty. Princeton, NJ, Princeton University Press, 1985.

Albert, Max. "Methodology and Scientific Competition". *Episteme*, volume 8, number 2, 2011, pages 165-183.
http://dx.doi.org/10.3366/epi.2011.0014

Alexander, Jason McKenzie, Johannes Himmelreich and Christopher Thompson. "Epistemic Landscapes, Optimal Search and the Division of Cognitive Labor". Preprint dated 30 November 2014 and archived on 7 December 2014 at *PhilSci Archive*.
http://philsci-archive.pitt.edu/id/eprint/11187

Alexander, Joshua. *Experimental Philosophy: An Introduction*. Cambridge, Polity Press, 2012.

Alexandrova, Anna, and Robert Northcott. “Progress in Economics: Lessons from the Spectrum Auctions”. Chapter 11 of Harold Kincaid and Don Ross (eds.), *The Oxford Handbook of Philosophy of Economics*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780195189254.003.0011

All Results Journals.
http://www.arjournals.com/

Alston, Lee J. “The ‘Case’ for Case Studies in New Institutional Economics”. Chapter 5 of Éric Brousseau and Jean-Michel Glachant (eds.), *New Institutional Economics: A Guidebook*. Cambridge, Cambridge University Press, 2008.

Altman, Morris (ed.). *Handbook of Contemporary Behavioral Economics: Foundations and Developments*. Armonk, NY, M. E. Sharpe, 2006.

Amitrano, David. “Variability in the Power-Law Distributions of Rupture Events: How and Why Does *b*-value Change?” *European Physical Journal Special Topics*, volume 205, number 1, 2012, pages 199-215.
http://dx.doi.org/10.1140/epjst/e2012-01571-9

Andersen, Hanne. “Joint Acceptance and Scientific Change: A Case Study”. *Episteme*, volume 7, number 3, 2010, pages 248-265.

Anderson, Jason. “Loss of Ecosystem Services Provided by Peat Bogs in the UK and Finland”. Annex 5 of Marianne Kettunen and Patrick ten Brink, *Value of Biodiversity: Documenting EU Examples Where Biodiversity Loss has led to the Loss of Ecosystem Services, Final Report for the European Commission*. Brussels, Institute for European Environmental Policy, 2006.
http://ec.europa.eu/environment/enveco/pdf/value_biodiversity.pdf

Andreeva, Antonina, Dave Howorth, Cyrus Chothia, Eugene Kulesha and Alexey G. Murzin. “SCOP2 Prototype: A New Approach to Protein Structure Mining”. *Nucleic Acids Research*, volume 42, number D1, 2014, pages D310-D314.
http://dx.doi.org/10.1093/nar/gkt1242

Antràs, Pol, and Hans-Joachim Voth. “Factor Prices and Productivity Growth During the British Industrial Revolution”. *Explorations in Economic History*, volume 40, number 1, 2003, pages 52-77.
http://dx.doi.org/10.1016/S0014-4983(02)00024-4

Arabatzis, Theodore. “Experiment”. Chapter 18 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (chapter 15 of the first edition, 2008).

Arabatzis, Theodore. “On the Inextricability of the Context of Discovery and the Context of Justification”. Chapter 13 of Jutta Schickore and Friedrich Steinle (eds.), *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*. Dordrecht, Springer, 2006.
http://dx.doi.org/10.1007/1-4020-4251-5_13

Arjas, Elja, and Mervi Eerola. "On Predictive Causality in Longitudinal Studies". *Journal of Statistical Planning and Inference*, volume 34, number 3, 1993, pages 361-386.
http://dx.doi.org/10.1016/0378-3758(93)90146-W

Arnold, Markus. "Disziplin und Initiation: Die kulturellen Praktiken der Wissenschaft". Chapter 1 of Markus Arnold and Roland Fischer (eds.), *Disziplinierungen: Kulturen der Wissenschaft im Vergleich*. Wien, Turia und Kant, 2004.

Arribas-Ayllon, Michael, and Valerie Walkerdine. "Foucauldian Discourse Analysis". Chapter 6 of Carla Willig and Wendy Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Artikis, Alexander, Robert Craven, Nihan Kesim Çiçekli, Babak Sadighi and Kostas Stathis (eds.). *Logic Programs, Norms and Action: Essays in Honor of Marek J. Sergot on the Occasion of His 60th Birthday*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-3-642-29414-3

Artmann, Stefan. "Biological Information". Chapter 2 of Sahotra Sarkar and Anya Plutynski (eds.), *A Companion to the Philosophy of Biology*. Oxford, Blackwell, 2008.

arXiv. An online repository for papers in mathematics and some of the natural sciences.
http://arxiv.org/

ATLAS Collaboration. "Observation of a New Particle in the Search for the Standard Model Higgs Boson with the ATLAS Detector at the LHC". *Physics Letters B*, volume 716, number 1, 2012, pages 1-29.
http://dx.doi.org/10.1016/j.physletb.2012.08.020

Axelson-Fisk, Marina. *Comparative Gene Finding: Models, Algorithms and Implementation*. Dordrecht, Springer, 2010. http://dx.doi.org/10.1007/978-1-84996-104-2

Axtell, Guy. "Bridging a Fault Line: On Underdetermination and the *Ampliative Adequacy* of Competing Theories". Chapter 14 of Abrol Fairweather (ed.), *Virtue Epistemology Naturalized: Bridges Between Virtue Epistemology and Philosophy of Science*. Dordrecht, Springer, 2014. http://dx.doi.org/10.1007/978-3-319-04672-3_14

Aydinonat, N. Emrah. *The Invisible Hand in Economics: How Economists Explain Unintended Social Consequences*. Abingdon, Routledge, 2008.

Azzouni, Jody. *Knowledge and Reference in Empirical Science*. London, Routledge, 2000.

Baddeley, Michelle. *Behavioural Economics and Finance*. Abingdon, Routledge, 2013.

Baetu, Tudor M. "Filling in the Mechanistic Details: Two-Variable Experiments as Tests for Constitutive Relevance". *European Journal for Philosophy of Science*, volume 2, number 3, 2012, pages 337-353. http://dx.doi.org/10.1007/s13194-011-0045-3

Bailey, Roy E. *The Economics of Financial Markets*. Cambridge, Cambridge University Press, 2005.

Baker, Lynne Rudder. "Third-Person Understanding". Chapter 8 of Anthony J. Sanford (ed.), *The Nature and Limits of Human Understanding*. London, Continuum, 2003.

Ballesteros, Fernando J., Vicent J. Martínez, Andrés Moya and Bartolo Luque. “Energy Balance and the Origin of Kleiber’s Law”, version 1, 14 July 2014.
http://arxiv.org/abs/1407.3659v1

Banaji, Mahzarin R., and Susan A. Gelman (eds.). *Navigating the Social World: What Infants, Children, and Other Species Can Teach Us*. Oxford, Oxford University Press, 2013.
http://dx.doi.org/10.1093/acprof:oso/9780199890712.001.0001

Bandyopadhyay, Prasanta S., and Malcolm R. Forster (eds.). *Philosophy of Statistics: Handbook of the Philosophy of Science, volume 7*. Amsterdam, North-Holland, 2011.
http://www.sciencedirect.com/science/book/9780444518620

Barahona, Ana, and Vladimir Cachón. “The Rhetorical Dimension of Stephen Jay Gould’s Work”. Chapter 25 of Michael Ruse (ed.), *The Oxford Handbook of Philosophy of Biology*. Oxford, Oxford University Press, 2008.

Barlow, Hugh D., and David Kauzlarich. *Explaining Crime: A Primer in Criminological Theory*. Lanham, MD, Rowman and Littlefield, 2010.

Barnard, Alan. “Emic and Etic”. Pages 220-223 of Alan Barnard and Jonathan Spencer (eds.), *The Routledge Encyclopedia of Social and Cultural Anthropology*, second edition. Abingdon, Routledge, 2010.

Barrett, Louise, S. Peter Henzi and David Lusseau. "Taking Sociality Seriously: The Structure of Multi-Dimensional Social Networks as a Source of Information for Individuals". *Philosophical Transactions of the Royal Society B*, volume 367, number 1599, 2012, pages 2108-2118.
http://dx.doi.org/10.1098/rstb.2012.0113

Barrow, John D. *Mathletics: A Scientist Explains 100 Amazing Things About Sport*. London, Vintage, 2013.

Bartling, Sönke, and Sascha Friesike (eds.). *Opening Science: The Evolving Guide on How the Internet is Changing Research, Collaboration and Scholarly Publishing*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-00026-8

Bartling, Sönke, and Sascha Friesike. "Towards Another Scientific Revolution". Chapter 1 of Bartling and Friesike (eds.), *Opening Science: The Evolving Guide on How the Internet is Changing Research, Collaboration and Scholarly Publishing*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-00026-8_1

Bates, Robert H., Avner Greif, Margaret Levi, Jean-Laurent Rosenthal and Barry R. Weingast. *Analytic Narratives*. Princeton, NJ, Princeton University Press, 1998.

Batterman, Robert (ed.). *The Oxford Handbook of Philosophy of Physics*. Oxford, Oxford University Press, 2013.
http://dx.doi.org/10.1093/oxfordhb/9780195392043.001.0001

Bauer, Alexander A. “Multivocality and ‘Wikiality’: The Epistemology and Ethics of a Pragmatic Archaeology”. Chapter 10 of Geoffrey Scarre and Robin Coningham (eds.), *Appropriating the Past: Philosophical Perspectives on the Practice of Archaeology*. Cambridge, Cambridge University Press, 2013.
http://dx.doi.org/10.1017/CBO9781139026932.012

Bauer, Andrej. “The HoTT Book”. *Mathematics and Computation* (blog), 20 June 2013.
http://math.andrej.com/2013/06/20/the-hott-book/

Beard, Mary. *The Roman Triumph*. Cambridge, MA, The Belknap Press of Harvard University Press, 2007.

Becker, Gary S. *A Treatise on the Family*, enlarged edition. Cambridge, MA, Harvard University Press, 1991.

Beckermann, Ansgar, and Christian Nimtz (eds.). *Argument und Analyse. Ausgewählte Sektionsvorträge des 4. internationalen Kongresses der Gesellschaft für Analytische Philosophie*, online publication of papers from a conference held in Bielefeld in September 2000. (This online collection differs from the printed volume of proceedings of the same conference: Moulines, Carlos Ulises, and Karl-Georg Niebergall (eds.), *Argument und Analyse*, Paderborn, Mentis, 2002.)
http://www.gap-im-netz.de/gap4Konf/Proceedings4/Proc.htm

Beisbart, Claus. “How Can Computer Simulations Produce New Knowledge?” *European Journal for Philosophy of Science*, volume 2, number 3, 2012, pages 395-434.
http://dx.doi.org/10.1007/s13194-012-0049-7

Bennett, Andrew. "Process Tracing and Causal Inference". Chapter 10 of Henry E. Brady and David Collier (eds.), *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, second edition. Lanham, MD, Rowman and Littlefield, 2010.

Bennett, Andrew, and Colin Elman. "Complex Causal Relations and Case Study Methods: The Example of Path Dependence". *Political Analysis*, volume 14, number 3, 2006, pages 250-267.
http://dx.doi.org/10.1093/pan/mpj020

Bennett, Maxwell R., and Peter M. S. Hacker. *Philosophical Foundations of Neuroscience*. Oxford, Blackwell, 2003.

Bensel, Richard Franklin. *The American Ballot Box in the Mid-Nineteenth Century*. Cambridge, Cambridge University Press, 2004.

Bentley, Michael (ed.). *Companion to Historiography*. London, Routledge, 1997.

Bernard, George W. "Law, Justice, and Governance: New Views on Medieval Constitutionalism". Chapter 13 of Richard W. Kaeuper (ed.), *Law, Governance, and Justice: New Views on Medieval Constitutionalism*. Leiden, Brill, 2013.

Bernard, H. Russell. *Research Methods in Anthropology: Qualitative and Quantitative Approaches*, fifth edition. Lanham, MD, AltaMira Press, 2011.

Bernecker, Sven, and Duncan Pritchard (eds.). *The Routledge Companion to Epistemology*. Abingdon, Routledge, 2011.

Berridge, Kent C. “Motivation Concepts in Behavioral Neuroscience”. *Physiology and Behavior*, volume 81, number 2, 2004, pages 179-209.
http://dx.doi.org/10.1016/j.physbeh.2004.02.004

Berridge, Kent C., and Morten L. Kringelbach. “Towards a Neuroscience of Well-Being: Implications of Insights from Pleasure Research”. Chapter 7 of Hilke Brockmann and Jan Delhey (eds.), *Human Happiness and the Pursuit of Maximization: Is More Always Better?* Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-6609-9_7

Berry, John W., Ype H. Poortinga, Seger M. Breugelmans, Athanasios Chasiotis and David L. Sam. *Cross-Cultural Psychology: Research and Applications*, third edition. Cambridge, Cambridge University Press, 2011 (second edition by John W. Berry, Ype H. Poortinga, Marshall H. Segall and Pierre R. Dasen, 2002).

Betz, Gregor. *Debate Dynamics: How Controversy Improves our Beliefs*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-4599-5

Bewley, Truman F. “Fairness, Reciprocity, and Wage Rigidity”. Chapter 5 of Peter Diamond and Hannu Vartiainen (eds.), *Behavioral Economics and its Applications*. Princeton, NJ, Princeton University Press, 2007.

Bickle, John (ed.). *The Oxford Handbook of Philosophy and Neuroscience*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780195304787.001.0001

Binmore, Ken. *Game Theory and the Social Contract. Volume 1: Playing Fair*. Cambridge, MA, MIT Press, 1994.

Binmore, Ken. *Game Theory and the Social Contract. Volume 2: Just Playing*. Cambridge, MA, MIT Press, 1998.

Binmore, Ken. *Natural Justice*. Oxford, Oxford University Press, 2005.

Biology Direct. Journal.
http://www.biologydirect.com/

Birch, Jonathan. "Robust Processes and Teleological Language". *European Journal for Philosophy of Science*, volume 2, number 3, 2012, pages 299-312.
http://dx.doi.org/10.1007/s13194-011-0043-5

Blackledge, Paul. *Reflections on the Marxist Theory of History*. Manchester, Manchester University Press, 2006.

Bloch, Maurice. *Anthropology and the Cognitive Challenge*. Cambridge, Cambridge University Press, 2012.
http://dx.doi.org/10.1017/CBO9781139020008

Blundell, Richard, Claire Crawford and Wenchao Jin. "What Can Wages and Employment Tell Us about the UK's Productivity Puzzle?" *The Economic Journal*, volume 124, number 576, 2014, pages 377-407.
http://dx.doi.org/10.1111/ecoj.12138

Bock, Walter J. "Multiple Explanations in Darwinian Evolutionary Theory". *Acta Biotheoretica*, volume 58, number 1, 2010, pages 65-79.
http://dx.doi.org/10.1007/s10441-009-9094-9

Bogdan, Radu J. "Common Sense Naturalized: The Practical Stance". Chapter 9 of Radu J. Bogdan (ed.), *Mind and Common Sense: Philosophical Essays on Commonsense Psychology*. Cambridge, Cambridge University Press, 1991.
http://dx.doi.org/10.1017/CBO9780511551659.009

Bogdan, Radu J. (ed.). *Mind and Common Sense: Philosophical Essays on Commonsense Psychology*. Cambridge, Cambridge University Press, 1991.
http://dx.doi.org/10.1017/CBO9780511551659

Boghossian, Paul A. *Fear of Knowledge: Against Relativism and Constructivism*. Oxford, Clarendon Press, 2006.

Bohannan, Laura. "Shakespeare in the Bush". Chapter 5 of James Spradley and David W. McCurdy (eds.), *Conformity and Conflict: Readings in Cultural Anthropology*, fourteenth edition. London, Pearson, 2011.

Bohannon, John. "Who's Afraid of Peer Review?" *Science*, volume 342, number 6154, 2013, pages 60-65.
http://dx.doi.org/10.1126/science.342.6154.60

Boix, Carles, and Susan C. Stokes (eds.). *The Oxford Handbook of Comparative Politics*. Oxford, Oxford University Press, 2007.
http://dx.doi.org/10.1093/oxfordhb/9780199566020.001.0001

Bokulich, Alisa. "Explanatory Models Versus Predictive Models: Reduced Complexity Modeling in Geomorphology". Chapter 10 of Vassilios Karakostas and Dennis Dieks (eds.), *EPSA 11 Perspectives and Foundational Problems in Philosophy of Science. The European Philosophy of Science Association Proceedings, volume 2*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-3-319-01306-0_10

Bold, Stefan, Benedikt Löwe, Thoralf Räsch and Johan van Benthem (eds.). *Foundations of the Formal Sciences V: Infinite Games*. London, College Publications, 2007.

Boniolo, Giovanni, Paolo Budinich and Majda Trobok (eds.). *The Role of Mathematics in Physical Sciences: Interdisciplinary and Philosophical Aspects*. Dordrecht, Springer, 2005.

BonJour, Laurence. *The Structure of Empirical Knowledge*. Cambridge, MA, Harvard University Press, 1985.

Bonk, Thomas. *Underdetermination: An Essay on Evidence and the Limits of Natural Knowledge*. Dordrecht, Springer, 2008.

Boote, David N., and Penny Beile. "Scholars Before Researchers: On the Centrality of the Dissertation Literature Review in Research Preparation". *Educational Researcher*, volume 34, number 6, 2005, pages 3-15.
http://dx.doi.org/10.3102/0013189X034006003

Borwein, Jonathan, and David Bailey. *Mathematics by Experiment: Plausible Reasoning in the 21st Century*, second edition. Wellesley, MA, A. K. Peters, 2008.

Boumans, Marcel. "Model-Based Consensus". Chapter 3 of Carlo Martini and Marcel Boumans (eds.), *Experts and Consensus in Social Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-08551-7_3

Bourdieu, Pierre. *La Distinction. Critique sociale du jugement*. Paris, Les Éditions de Minuit, 1979.

Bourdieu, Pierre. *Distinction: A Social Critique of the Judgement of Taste*, translated by Richard Nice. Cambridge, MA, Harvard University Press, 1984.

Bovens, Luc, and Stephan Hartmann. “Belief Expansion, Contextual Fit, and the Reliability of Information Sources”. Chapter 34 of Varol Akman, Paolo Bouquet, Richmond Thomason and Roger A. Young (eds.), *Modeling and Using Context: Third International and Interdisciplinary Conference, CONTEXT 2001*. Berlin, Springer, 2001.
http://dx.doi.org/10.1007/3-540-44607-9_34

Bovens, Luc, and Stephan Hartmann. “Solving the Riddle of Coherence”. *Mind*, volume 112, number 448, 2003, pages 601-633.
http://dx.doi.org/10.1093/mind/112.448.601

Box-Steffensmeier, Janet M., Henry E. Brady and David Collier (eds.). *The Oxford Handbook of Political Methodology*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/oxfordhb/9780199286546.001.0001

Bracke, Thierry, and Michael Fidora. *Global Liquidity Glut or Global Savings Glut? A Structural VAR Approach*. Frankfurt am Main, European Central Bank, Working Paper number 911, June 2008.
http://www.ecb.europa.eu/pub/pdf/scpwps/ecbwp911.pdf

Brady, Henry E., and David Collier (eds.). *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, second edition. Lanham, MD, Rowman and Littlefield, 2010.

Brams, Steven J. *Game Theory and the Humanities: Bridging Two Worlds*. Cambridge, MA, MIT Press, 2011.

Brand, Jennie E., and Juli Simon Thomas. “Job Displacement among Single Mothers: Effects on Children’s Outcomes in Young Adulthood”. *American Journal of Sociology*, volume 119, number 4, 2014, pages 955-1001.
http://dx.doi.org/10.1086/675409

Brennan, Geoffrey, and Philip Pettit. "The Hidden Economy of Esteem". *Economics and Philosophy*, volume 16, number 1, 2000, pages 77-98.

Brigandt, Ingo. "Beyond Reduction and Pluralism: Toward an Epistemology of Explanatory Integration in Biology". *Erkenntnis*, volume 73, number 3, 2010, pages 295-311.
http://dx.doi.org/10.1007/s10670-010-9233-3

Brigandt, Ingo. "From Developmental Constraint to Evolvability: How Concepts Figure in Explanation and Disciplinary Identity". Chapter 14 of Alan C. Love (ed.), *Conceptual Change in Biology: Scientific and Philosophical Perspectives on Evolution and Development*. Dordrecht, Springer, 2015.
http://dx.doi.org/10.1007/978-94-017-9412-1_14

Brigandt, Ingo. "Systems Biology and the Integration of Mechanistic Explanation and Mathematical Explanation". *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, volume 44, number 4, part A, 2013, pages 477-492.
http://dx.doi.org/10.1016/j.shpsc.2013.06.002

Briggle, Adam, and Clifford G. Christians. "Media and Communication". Chapter 15 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Brockmann, Hilke, and Jan Delhey (eds.). *Human Happiness and the Pursuit of Maximization: Is More Always Better?* Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-6609-9

Brooks, James F., Christopher R. N. DeCorse and John Walton (eds.). *Small Worlds: Method, Meaning, and Narrative in Microhistory*. Santa Fe, NM, School for Advanced Research Press, 2008.

Brousseau, Éric, and Jean-Michel Glachant (eds.). *New Institutional Economics: A Guidebook*. Cambridge, Cambridge University Press, 2008.

Brown, Callum G. "'The Unholy Mrs Knight' and the BBC: Secular Humanism and the Threat to the 'Christian Nation', c.1945-60". *English Historical Review*, volume 127, number 525, 2012, pages 345-376.
http://dx.doi.org/10.1093/ehr/ces001

Brown, James Robert. "The Community of Science®". Chapter 9 of Martin Carrier, Don Howard and Janet A. Kourany (eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*. Pittsburgh, PA, University of Pittsburgh Press, 2008.

Brown, Susan, and John Simpson. "The Curious Identity of Michael Field and its Implications for Humanities Research with the Semantic Web". *2013 IEEE International Conference on Big Data*. pages 77-85.
http://dx.doi.org/10.1109/BigData.2013.6691674

Bruun, Hans Henrik. *Science, Values and Politics in Max Weber's Methodology*, new expanded edition. Aldershot, Ashgate, 2007.

Bryman, Alan. *Social Research Methods*, fourth edition. Oxford, Oxford University Press, 2012.

Buchman, Timothy G. "The Community of the Self". *Nature*, volume 420, number 6912, 2002, pages 246-251.
http://dx.doi.org/10.1038/nature01260

Buckling, Angus, R. Craig Maclean, Michael A. Brockhurst and Nick Colegrave. “The *Beagle* in a Bottle”. *Nature*, volume 457, number 7231, 2009, pages 824-829.
http://dx.doi.org/10.1038/nature07892

Buekens, Filip. “Faultless Disagreement, Assertions and the Affective-Expressive Dimension of Judgments of Taste”. *Philosophia*, volume 39, number 4, 2011, pages 637-655.
http://dx.doi.org/10.1007/s11406-011-9318-5

Buekens, Filip, and F. A. Muller. “Intentionality versus Constructive Empiricism”. *Erkenntnis*, volume 76, number 1, 2012, pages 91-100.
http://dx.doi.org/10.1007/s10670-011-9348-1

Bueno, Otávio, and Øystein Linnebo (eds.). *New Waves in Philosophy of Mathematics*. Basingstoke, Palgrave Macmillan, 2009.

Bulhof, Ilse N. *Wilhelm Dilthey: A Hermeneutic Approach to the Study of History and Culture*. The Hague, Martinus Nijhoff, 1980.

Buller, David J. “Evolutionary Psychology: The Emperor’s New Paradigm”. *Trends in Cognitive Sciences*, volume 9, number 6, 2005, pages 277-283.
http://dx.doi.org/10.1016/j.tics.2005.04.003
Erratum at http://dx.doi.org/10.1016/j.tics.2005.06.006

Burge, Tyler. “Computer Proof, Apriori Knowledge, and Other Minds”. Chapter 13 of Burge, *Cognition Through Understanding: Self-Knowledge, Interlocution, Reasoning, Reflection. Philosophical Essays, Volume 3*. Oxford, Oxford University Press, 2013.

Burge, Tyler. "Epistemic Warrant: Humans and Computers". Chapter 20 of Burge, *Cognition Through Understanding: Self-Knowledge, Interlocution, Reasoning, Reflection. Philosophical Essays, Volume 3*. Oxford, Oxford University Press, 2013.

Burggren, Warren, Kent Chapman, Bradley Keller, Michael Monticino and John Torday. "Biological Sciences". Chapter 8 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Burguière, André. *L'École des Annales. Une histoire intellectuelle*. Paris, Odile Jacob, 2006.

Burguière, André. *The Annales School: An Intellectual History*, translated by Jane Marie Todd. Ithaca, NY, Cornell University Press, 2009.

Burke, Peter (ed.). *New Perspectives on Historical Writing*, second edition. Cambridge, Polity Press, 2001.

Byrne, Ruth M. J. *The Rational Imagination: How People Create Alternatives to Reality*. Cambridge, MA, MIT Press, 2005.

Cabrera, Miguel A. *Postsocial History: An Introduction*, translated by Marie McMahon. Lanham, MD, Lexington Books, 2004.

Calcott, Brett. "Why How and Why Aren't Enough: More Problems With Mayr's Proximate-Ultimate Distinction". *Biology and Philosophy*, volume 28, number 5, 2013, pages 767-780.
http://dx.doi.org/10.1007/s10539-013-9367-1

Calhoun, Craig, and Diana Rhoten. “Integrating the Social Sciences: Theoretical Knowledge, Methodological Tools, and Practical Applications”. Chapter 7 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Campanario, Juan Miguel. “Rejecting and Resisting Nobel Class Discoveries: Accounts by Nobel Laureates”. *Scientometrics*, volume 81, number 2, 2009, pages 549-565. http://dx.doi.org/10.1007/s11192-008-2141-5

Cannadine, David. “The Present and the Past in the English Industrial Revolution 1880-1980”. *Past and Present*, volume 103, number 1, 1984, pages 131-172. http://dx.doi.org/10.1093/past/103.1.131

Capaldi, Nicholas. *John Stuart Mill: A Biography*. Cambridge, Cambridge University Press, 2004.

Caplin, Andrew, and Andrew Schotter (eds.). *The Foundations of Positive and Normative Economics: A Handbook*. Oxford, Oxford University Press, 2008. http://dx.doi.org/10.1093/acprof:oso/9780195328318.001.0001

Cappelen, Herman, and John Hawthorne. *Relativism and Monadic Truth*. Oxford, Oxford University Press, 2009.

Carr, Philip J., and Andrew P. Bradbury. “Flake Debris and Flintknapping Experimentation”. Chapter 4 of Jeffrey R. Ferguson (ed.), *Designing Experimental Research in Archaeology: Examining Technology Through Production and Use*. Boulder, CO, University Press of Colorado, 2010.

Carrier, Martin, Don Howard and Janet A. Kourany (eds.). *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited.* Pittsburgh, PA, University of Pittsburgh Press, 2008.

Cartier, Pierre. "Mathemagics (A Tribute to L. Euler and R. Feynman)". Second introductory chapter of Michel Planat (ed.), *Noise, Oscillators and Algebraic Randomness: From Noise in Communication Systems to Number Theory. Lectures of a School Held in Chapelle des Bois, France, April 5-10, 1999.* Berlin, Springer, 2000.

Cartwright, Nancy. *Hunting Causes and Using Them: Approaches in Philosophy and Economics.* Cambridge, Cambridge University Press, 2007.

Cartwright, Nancy, and Eileen Munro. "The Limitations of Randomized Controlled Trials in Predicting Effectiveness". *Journal of Evaluation in Clinical Practice*, volume 16, number 2, 2010, pages 260-266.
http://dx.doi.org/10.1111/j.1365-2753.2010.01382.x

Case Studies in Engineering Failure Analysis. Journal. eISSN 2213-2902.
http://www.journals.elsevier.com/case-studies-in-engineering-failure-analysis/

Caspers, Martijn, Chris Heunen, Nicolaas P. Landsman and Bas Spitters. "Intuitionistic Quantum Logic of an n-level System". *Foundations of Physics*, volume 39, number 7, 2009, pages 731-759.
http://dx.doi.org/10.1007/s10701-009-9308-7

Chakravartty, Anjan. "What You Don't Know Can't Hurt You: Realism and the Unconceived". *Philosophical Studies*, volume 137, number 1, 2008, pages 149-158.
http://dx.doi.org/10.1007/s11098-007-9173-1

Chalmers, Alan, and Deborah G. Mayo. “The Life of Theory in the New Experimentalism”. Chapter 2 of Deborah G. Mayo and Aris Spanos (eds.), *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*. Cambridge, Cambridge University Press, 2009.

Chang, Hasok. “Beyond Case-Studies: History as Philosophy”. Chapter 8 of Seymour Mauskopf and Tad Schmaltz (eds.), *Integrating History and Philosophy of Science: Problems and Prospects*. Dordrecht, Springer, 2012.

Chang, Hasok. *Is Water H_2O? Evidence, Realism and Pluralism*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-3932-1

Chao, Hsiang-Ke, Szu-Ting Chen and Roberta L. Millstein (eds.). *Mechanism and Causality in Biology and Economics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-2454-9

Chemero, Anthony. *Radical Embodied Cognitive Science*. Cambridge, MA, MIT press, 2009.

Chen, Caroline. “The Paradox of the Proof”. *Project Wordsworth* (blog), 9 May 2013.
http://projectwordsworth.com/the-paradox-of-the-proof/

Chen, Jinzhu, Prakash Kannan, Prakash Loungani and Bharat Trehan. *New Evidence on Cyclical and Structural Sources of Unemployment*. Washington, DC, International Monetary Fund, Working Paper number WP/11/106, May 2011.
http://www.imf.org/external/pubs/ft/wp/2011/wp11106.pdf

Chen, William W., Mario Niepel and Peter K. Sorger. "Classic and Contemporary Approaches to Modeling Biochemical Reactions". *Genes and Development*, volume 24, number 17, 2010, pages 1861-1875.
http://dx.doi.org/10.1101/gad.1945410

Cherry, John, Chris Scarre and Stephen Shennan (eds.). *Explaining Social Change: Studies in Honour of Colin Renfrew*. Cambridge, McDonald Institute for Archaeological Research, 2004.

Churchland, Paul M. "Folk Psychology and the Explanation of Human Behavior". Chapter 3 of Radu J. Bogdan (ed.), *Mind and Common Sense: Philosophical Essays on Commonsense Psychology*. Cambridge, Cambridge University Press, 1991.
http://dx.doi.org/10.1017/CBO9780511551659.003

Churchland, Paul M. "The Neural Representation of the Social World". Chapter 5 of Larry May, Marilyn Friedman and Andy Clark (eds.), *Mind and Morals: Essays on Ethics and Cognitive Science*. (Alternative title: *Mind and Morals: Essays on Cognitive Science and Ethics*.)

Clark, Christopher. *The Sleepwalkers: How Europe Went to War in 1914*. London, Allen Lane, 2012.

Clark, Stuart. "The *Annales* Historians". Chapter 10 of Quentin Skinner (ed.), *The Return of Grand Theory in the Human Sciences*. Cambridge, Cambridge University Press, 1985.

Clark, William A. V. "Residential Preferences and Neighborhood Racial Segregation: A Test of the Schelling Segregation Model". *Demography*, volume 28, number 1, 1991, pages 1-19.
http://dx.doi.org/10.2307/2061333

Clay Mathematics Institute. *IUT Theory of Shinichi Mochizuki*. Announcement of a workshop in December 2015.
http://www.claymath.org/events/iut-theory-shinichi-mochizuki

Clements, Michael P., and David F. Hendry (eds.). *The Oxford Handbook of Economic Forecasting*. Oxford, Oxford University Press, 2011.
http://dx.doi.org/10.1093/oxfordhb/9780195398649.001.0001

Clifford, William Kingdon. "The Ethics of Belief". Volume 2, chapter 5 of Clifford, *Lectures and Essays*, edited by Leslie Stephen and Sir Frederick Pollock, two volumes. London, Macmillan, 1901.

Cliodynamics: The Journal of Quantitative History and Cultural Evolution.
http://escholarship.org/uc/irows_cliodynamics

Cliometrica: Journal of Historical Economics and Econometric History. ISSN 1863-2505, eISSN 1863-2513.
http://link.springer.com/journal/11698

CMS Collaboration. "Observation of a New Boson at a Mass of 125 GeV with the CMS Experiment at the LHC". *Physics Letters B*, volume 716, number 1, 2012, pages 30-61.
http://dx.doi.org/10.1016/j.physletb.2012.08.021

Coe, Angela L. (ed.). *Geological Field Techniques*. Oxford, Blackwell, 2010.

Colander, David, Richard Holt and Barkley Rosser Jr. "The Changing Face of Mainstream Economics". *Review of Political Economy*, volume 16, number 4, 2004, pages 485-499.
http://dx.doi.org/10.1080/0953825042000256702

Cole, Julian C. "Creativity, Freedom, and Authority: A New Perspective on the Metaphysics of Mathematics". *Australasian Journal of Philosophy*, volume 87, number 4, 2009, pages 589-608.
http://dx.doi.org/10.1080/00048400802598629

Collini, Stefan, Donald Winch and John Burrow. *That Noble Science of Politics: A Study in Nineteenth-Century Intellectual History*. Cambridge, Cambridge University Press, 1983.

Colman, Andrew M. "Love is not Enough: Other-Regarding Preferences Cannot Explain Payoff Dominance in Game Theory". *Behavioral and Brain Sciences*, volume 30, number 1, 2007, pages 22-23.
http://dx.doi.org/10.1017/S0140525X07000659

Comte, Auguste. *Cours de philosophie positive*, tome 1. Paris, Rouen Frères, 1830.

Comte, Auguste. *The Essential Comte: Selected from Cours de Philosophie Positive*, edited and with an introduction by Stanislav Andreski and translated and annotated by Margaret Clarke. London, Croom Helm, 1974.

Confer, Jaime C., Judith A. Easton, Diana S. Fleischman, Cari D. Goetz, David M. G. Lewis, Carin Perilloux and David M. Buss. "Evolutionary Psychology: Controversies, Questions, Prospects, and Limitations". *American Psychologist*, volume 65, number 2, 2010, pages 110-126.
http://dx.doi.org/10.1037/a0018413

Connor, James. *The Sociology of Loyalty*. New York, NY, Springer, 2007.
http://dx.doi.org/10.1007/978-0-387-71368-7

Cope, Bill, and Angus Phillips (eds.). *The Future of the Academic Journal*, second edition. Kidlington, Chandos, 2014.

Costume. Journal of the Costume Society. ISSN 0590-8876, eISSN 1749-6306.
http://www.maneyonline.com/loi/cos

Couto-Cantero, Pilar, Gonzalo Enríquez Veloso, Alberta Passeri and José María Paz Gago (eds.), *Culture of Communication – Communication of Culture: Proceedings of the 10th World Congress of the International Association for Semiotic Studies*, online publication. A Coruña, Universidade da Coruña, 2012.
http://dialnet.unirioja.es/servlet/libro?codigo=515368

Couzin-Frankel, Jennifer. "The Power of Negative Thinking". *Science*, volume 342, number 6154, 2013, pages 68-69.
http://dx.doi.org/10.1126/science.342.6154.68

Cox, Philip, Beryl Plimmer and Peter Rodgers (eds.). *Diagrammatic Representation and Inference: 7th International Conference, Diagrams 2012*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-3-642-31223-6

Cramer, Christopher J. *Essentials of Computational Chemistry: Theories and Models*, second edition. Chichester, Wiley, 2004.

Crang, Mike, and Nigel J. Thrift (eds.). *Thinking Space*. London, Routledge, 2000.

Crasnow, Sharon. “The Role of Case Study Research in Political Science: Evidence for Causal Claims”. *Philosophy of Science*, volume 79, number 5, 2012, pages 655-666.
http://dx.doi.org/10.1086/667869

Craver, Carl F. *Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience*. Oxford, Clarendon Press, 2007.

Crease, Robert P. “Physical Sciences”. Chapter 6 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Cunliffe, Barry, Chris Gosden and Rosemary A. Joyce (eds.). *The Oxford Handbook of Archaeology*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780199271016.001.0001

Curd, Martin, and Stathis Psillos (eds.). *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (first edition 2008).

Current Protocols. Journal.
http://www.currentprotocols.com

Czarnecki, Tadeusz, Katarzyna Kijania-Placek, Olga Poller and Jan Woleński (eds.). *The Analytical Way: Proceedings of the 6th European Congress of Analytic Philosophy*. London, College Publications, 2010.

Da Costa, Newton C. A., and Steven French. *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*. Oxford, Oxford University Press, 2003.

Darden, Lindley. *Reasoning in Biological Discoveries: Essays on Mechanisms, Interfield Relations, and Anomaly Resolution*. Cambridge, Cambridge University Press, 2006.
http://dx.doi.org/10.1017/CBO9780511498442

Data Mining and Knowledge Discovery. Journal. ISSN 1384-5810, eISSN 1573-756X.
http://link.springer.com/journal/10618

Davey, Kevin J. "Is Mathematical Rigor Necessary in Physics?" *British Journal for the Philosophy of Science*, volume 54, number 3, 2003, pages 439-463.
http://dx.doi.org/10.1093/bjps/54.3.439

Davey, Kevin J. *Problems in Applying Mathematics: On the Inferential and Representational Limits of Mathematics in Physics*. PhD thesis, University of Pittsburgh, 2003.
http://d-scholarship.pitt.edu/9534/1/kdavey2003.pdf

Davidson, Donald. "On the Very Idea of a Conceptual Scheme". Essay 13 of Davidson, *Inquiries into Truth and Interpretation*, second edition. Oxford, Clarendon Press, 2001.

Davidson, Ian. *Voltaire: A Life*, revised edition. London, Profile Books, 2012.

Davies, C. S. L. "Tudor: What's in a Name?" *History*, volume 97, number 325, 2012, pages 24-42.
http://dx.doi.org/10.1111/j.1468-229X.2011.00540.x

Davis, John B. "Competing Conceptions of the Individual in Recent Economics". Chapter 8 of Harold Kincaid and Don Ross (eds.), *The Oxford Handbook of Philosophy of Economics*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780195189254.003.0008

Davis, John B. “The Turn in Recent Economics and Return of Orthodoxy”. *Cambridge Journal of Economics*, volume 32, number 3, 2008, pages 349-366.
http://dx.doi.org/10.1093/cje/bem048

Dawid, Richard, Stephan Hartmann and Jan Sprenger. “The No Alternatives Argument”. *British Journal for the Philosophy of Science*, volume 66, number 1, 2015, pages 213-234.
http://dx.doi.org/10.1093/bjps/axt045

De Cruz, Helen, and Johan De Smedt. “Evolved Cognitive Biases and the Epistemic Status of Scientific Beliefs”. *Philosophical Studies*, volume 157, number 3, 2012, pages 411-429.
http://dx.doi.org/10.1007/s11098-010-9661-6

De Cruz, Helen, and Johan De Smedt. “The Role of Intuitive Ontologies in Scientific Understanding – The Case of Human Evolution”. *Biology and Philosophy*, volume 22, number 3, 2007, pages 351-368.
http://dx.doi.org/10.1007/s10539-006-9036-8

De Cruz, Helen, and Johan De Smedt. “The Value of Epistemic Disagreement in Scientific Practice: The Case of *Homo floresiensis*”. *Studies in History and Philosophy of Science Part A*, volume 44, number 2, 2013, pages 169-177.
http://dx.doi.org/10.1016/j.shpsa.2013.02.002

De Langhe, Rogier. “Mainstream Economics: Searching Where the Light is”. *Journal of Post Keynesian Economics*, volume 32, number 1, 2009, pages 137-150.
http://dx.doi.org/10.2753/PKE0160-3477320109

De Langhe, Rogier. "The Division of Labour in Science: The Tradeoff Between Specialisation and Diversity". *Journal of Economic Methodology*, volume 17, number 1, 2010, pages 37-51.
http://dx.doi.org/10.1080/13501780903528960

De Langhe, Rogier. "To Specialize or to Innovate: An Internalist Account of Pluralistic Ignorance in Economics". *Synthese*, volume 191, number 11, 2014, pages 2499-2511.
http://dx.doi.org/10.1007/s11229-014-0436-z

De Marchi, Scott. *Computational and Mathematical Modeling in the Social Sciences*. Cambridge, Cambridge University Press, 2005.

De Mey, Tim. "Imagination's Grip on Science". *Metaphilosophy*, volume 37, number 2, 2006, pages 222-239.
http://dx.doi.org/10.1111/j.1467-9973.2006.00432.x

De Queiroz, Kevin. "Ernst Mayr and the Modern Concept of Species". *Proceedings of the National Academy of Sciences of the United States of America*, volume 102, supplement 1, 2005, pages 6600-6607.
http://dx.doi.org/10.1073/pnas.0502030102

De Regt, Henk W., and Dennis Dieks. "A Contextual Approach to Scientific Understanding". *Synthese*, volume 144, number 1, 2005, pages 137-170.
http://dx.doi.org/10.1007/s11229-005-5000-4

De Regt, Henk W., Stephan Hartmann and Samir Okasha (eds.). *EPSA Philosophy of Science: Amsterdam 2009. The European Philosophy of Science Association Proceedings, volume 1*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-2404-4

De Ridder, Jeroen. "Epistemic Dependence and Collective Scientific Knowledge". *Synthese*, volume 191, number 1, 2014, pages 37-53.
http://dx.doi.org/10.1007/s11229-013-0283-3

De Ste. Croix, Geoffrey E. M. *The Class Struggle in the Ancient Greek World: from the Archaic Age to the Arab Conquests*. London, Duckworth, 1981.

Dekkers, Olaf M., Erik von Elm, Ale Algra, Johannes A. Romijn and Jan P. Vandenbroucke. "How to Assess the External Validity of Therapeutic Trials: A Conceptual Approach". *International Journal of Epidemiology*, volume 39, number 1, 2010, pages 89-94.
http://dx.doi.org/10.1093/ije/dyp174

Della Porta, Donatella, and Michael Keating (eds.). *Approaches and Methodologies in the Social Sciences: A Pluralist Perspective*. Cambridge, Cambridge University Press, 2008.

Dellantonio, Sara. "Sinneswahrnehmung und Überzeugungen. Die Frage nach dem empirischen Fundament". Chapter 13 of Ansgar Beckermann and Christian Nimtz (eds.), *Argument und Analyse. Ausgewählte Sektionsvorträge des 4. internationalen Kongresses der Gesellschaft für Analytische Philosophie*, online publication of papers from a conference held in Bielefeld in September 2000. (This online collection differs from the printed volume of proceedings of the same conference: Moulines, Carlos Ulises, and Karl-Georg Niebergall (eds.), *Argument und Analyse*, Paderborn, Mentis, 2002.)
http://www.gap-im-netz.de/gap4Konf/Proceedings4/pdf/6EK02Dellantonio.pdf

Dennett, Daniel C. *The Intentional Stance*. Cambridge, MA, MIT Press, 1987.

D'Errico, Francesco, and William E. Banks. "Identifying Mechanisms behind Middle Paleolithic and Middle Stone Age Cultural Trajectories". *Current Anthropology*, volume 54, number S8, 2013, pages S371-S387.
http://dx.doi.org/10.1086/673388

Diamond, Arthur M., Jr. "Science as a Rational Enterprise". *Theory and Decision*, volume 24, number 2, 1988, pages 147-167.
http://dx.doi.org/10.1007/BF00132459

Diamond, Jared M. "Horrible Plant Species". *Nature*, volume 360, number 6405, 1992, pages 627-628.
http://dx.doi.org/10.1038/360627a0

Diamond, Peter, and Hannu Vartiainen (eds.). *Behavioral Economics and its Applications*. Princeton, NJ, Princeton University Press, 2007.

Dickhaus, Thorsten. *Simultaneous Statistical Inference With Applications in the Life Sciences*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-642-45182-9

Diebold, Francis X. *Elements of Forecasting*, fourth edition. Mason, OH, Thomson South-Western, 2007.

Dieks, Dennis. "The Flexibility of Mathematics". Chapter 8 of Giovanni Boniolo, Paolo Budinich and Majda Trobok (eds.), *The Role of Mathematics in Physical Sciences: Interdisciplinary and Philosophical Aspects*. Dordrecht, Springer, 2005.

Dieks, Dennis, Wenceslao J. González, Stephan Hartmann, Michael Stöltzner and Marcel Weber (eds.). *Probabilities, Laws, and Structures*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-3030-4

Dieks, Dennis, Wenceslao J. González, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.). *Explanation, Prediction, and Confirmation*. Dordrecht, Springer, 2011.
http://dx.doi.org/10.1007/978-94-007-1180-8

Dietz, Richard, and Sebastiano Moruzzi (eds.). *Cuts and Clouds: Vagueness, its Nature, and its Logic*. Oxford, Oxford University Press, 2010.

Digital Scholarship in the Humanities (*Literary and Linguistic Computing* up to 2014). Journal of the European Association for Digital Humanities and other bodies. ISSN 2055-7671, eISSN 2055-768X.
http://dsh.oxfordjournals.org/

Dilthey, Wilhelm. "Der Aufbau der geschichtlichen Welt in den Geisteswissenschaften". Part 2 of Dilthey, *Der Aufbau der geschichtlichen Welt in den Geisteswissenschaften* (Gesammelte Schriften, Band 7, 8. Auflage). Stuttgart, B. G. Teubner, 1992.

Dilthey, Wilhelm. "Die Kategorien des Lebens". Part 3.1.3 of Dilthey, *Der Aufbau der geschichtlichen Welt in den Geisteswissenschaften* (Gesammelte Schriften, Band 7, 8. Auflage). Stuttgart, B. G. Teubner, 1992.

Dilthey, Wilhelm. *The Formation of the Historical World in the Human Sciences: Selected Works, Volume 3*, edited by Rudolf A. Makkreel and Frithjof Rodi. Princeton, NJ, Princeton University Press, 2002. This volume includes translations of the works listed above, under the same part numbers: "The Formation of the Historical World in the Human Sciences" (part 2); "The Categories of Life" (part 3.1.3).

Dion, Douglas. "Evidence and Inference in the Comparative Case Study". *Comparative Politics*, volume 30, number 2, 1998, pages 127-145.
http://dx.doi.org/10.2307/422284

Dixon, Huw David. "Keeping up with the Joneses: Competition and the Evolution of Collusion". *Journal of Economic Behavior and Organization*, volume 43, number 2, 2000, pages 223-238.
http://dx.doi.org/10.1016/S0167-2681(00)00117-7

Dizadji-Bahmani, Foad, Roman Frigg and Stephan Hartmann. "Who's Afraid of Nagelian Reduction?" *Erkenntnis*, volume 73, number 3, 2010, pages 393-412.
http://dx.doi.org/10.1007/s10670-010-9239-x

Dobson, Miriam, and Benjamin Ziemann (eds.). *Reading Primary Sources: The Interpretation of Texts from Nineteenth- and Twentieth-Century History*. Abingdon, Routledge, 2009.

Dodd, Gwilym. "Was Thomas Favent a Political Pamphleteer? Faction and Politics in Later Fourteenth-Century London". *Journal of Medieval History*, volume 37, number 4, 2011, pages 397-418.
http://dx.doi.org/10.1016/j.jmedhist.2011.09.003

Doppelt, Gerald. “Values in Science”. Chapter 32 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (chapter 28 of the first edition, 2008).

Dorato, Mauro. “Mathematical Biology and the Existence of Biological Laws”. Chapter 8 of Dennis Dieks, Wenceslao J. González, Stephan Hartmann, Michael Stöltzner and Marcel Weber (eds.), *Probabilities, Laws, and Structures*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-3030-4_8

Douglas, Heather. *Science, Policy, and the Value-Free Ideal*. Pittsburgh, PA, University of Pittsburgh Press, 2009.

Douglas, Mary. *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*, Routledge Classics edition. London, Routledge, 2002.

Douven, Igor, and Wouter Meijs. “Measuring Coherence”. *Synthese*, volume 156, number 3, 2007, pages 405-425.
http://dx.doi.org/10.1007/s11229-006-9131-z

Douven, Igor, and Alexander Riegler. “Extending the Hegselmann-Krause Model I”. *Logic Journal of the IGPL*, volume 18, number 2, 2010, pages 323-335.
http://dx.doi.org/10.1093/jigpal/jzp059

Dowson, Duncan. *History of Tribology*, second edition. London, Professional Engineering Publishing, 1998.

Doyle, William. *Origins of the French Revolution*, third edition. Oxford, Oxford University Press, 1999.

Du Plessis, Stan. “The Miracle of the Septuagint and the Promise of Data Mining in Economics”. Chapter 16 of Harold Kincaid and Don Ross (eds.), *The Oxford Handbook of Philosophy of Economics*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780195189254.003.0016

Ducheyne, Steffen. “Some Worries for Norton’s Material Theory of Induction”. *Philosophia Naturalis*, volume 45, number 1, 2008, pages 37-46.
http://dx.doi.org/10.3196/003180208787332387

Duhem, Pierre. *La Théorie physique. Son objet et sa structure*. Paris, Chevalier et Rivière, 1906.

Duhem, Pierre. *The Aim and Structure of Physical Theory*, translated by Philip P. Wiener. Princeton, NJ, Princeton University Press, 1954. (The translation is of the second edition, published in 1914, but the differences between the 1906 and the 1914 editions are not relevant to the use made of the book in this work.)

Durrenberger, E. Paul, and Kathleen Gillogly. “Greed in a ‘Tribal’ Economy? Acquisitiveness and Reciprocity in Lisu Society”. *Economic Anthropology*, volume 1, number 1, 2014, pages 88-103.
http://onlinelibrary.wiley.com/doi/10.1002/sea2.12006/abstract

Dutilh Novaes, Catarina. “A Dialogical Account of Deductive Reasoning as a Case Study for how Culture Shapes Cognition”. *Journal of Cognition and Culture*, volume 13, number 5, 2013, pages 459-482.
http://dx.doi.org/10.1163/15685373-12342104

Dwan, Kerry, Douglas G. Altman, Juan A. Arnaiz, Jill Bloom, An-Wen Chan, Eugenia Cronin, Evelyne Decullier, Philippa J. Easterbrook, Erik Von Elm, Carrol Gamble, Davina Ghersi, John P. A. Ioannidis, John Simes and Paula R. Williamson. "Systematic Review of the Empirical Evidence of Study Publication Bias and Outcome Reporting Bias". *PLOS ONE*, volume 3, number 8, 2008, e3081, pages 1-31.
http://dx.doi.org/10.1371/journal.pone.0003081

Easwaran, Kenny. "Probabilistic Proofs and Transferability". *Philosophia Mathematica*, volume 17, number 3, 2009, pages 341-362.
http://dx.doi.org/10.1093/philmat/nkn032

Easwaran, Kenny. "The Role of Axioms in Mathematics". *Erkenntnis*, volume 68, number 3, 2008, pages 381-391.
http://dx.doi.org/10.1007/s10670-008-9106-1

The Economic History Review. Journal of the Economic History Society. ISSN 0013-0117, eISSN 1468-0289.
http://onlinelibrary.wiley.com/journal/10.1111/(ISSN)1468-0289

The Economist Intelligence Unit. *Democracy Index 2014: Democracy and its Discontents*. London, The Economist Intelligence Unit, 2015.

Egg, Matthias. "Expanding Our Grasp: Causal Knowledge and the Problem of Unconceived Alternatives". *British Journal for the Philosophy of Science*, advance online publication, October 2014.
http://dx.doi.org/10.1093/bjps/axu025

Eisen, Michael. "I Confess, I Wrote the Arsenic DNA Paper to Expose Flaws in Peer-Review at Subscription Based Journals". *it is NOT junk* (blog), 3 October 2013. http://www.michaeleisen.org/blog/?p=1439.

Elgin, Catherine Z. *Considered Judgment*. Princeton, NJ, Princeton University Press, 1996.

Elliott, Jane. *Using Narrative in Social Research: Qualitative and Quantitative Approaches*. London, Sage, 2005.

Elliott, John H. *History in the Making*. New Haven, CT, Yale University Press, 2012.

Elsner, James B. "Granger Causality and Atlantic Hurricanes". *Tellus*, volume 59, number 4, 2007, pages 476-485. http://dx.doi.org/10.1111/j.1600-0870.2007.00244.x

Elton, Geoffrey R. *The Practice of History*, second edition with an afterword by Richard J. Evans. Oxford, Blackwell, 2002.

Elwert, Felix. "Graphical Causal Models". Chapter 13 of Stephen L. Morgan (ed.), *Handbook of Causal Analysis for Social Research*. Dordrecht, Springer, 2013. http://dx.doi.org/10.1007/978-94-007-6094-3_13

Ely, Richard G. "Mandelbaum on Historical Narrative: A Discussion". Chapter 4 of Geoffrey Roberts (ed.), *The History and Narrative Reader*. London, Routledge, 2001.

Endicott, Ronald. "Reinforcing the Three 'R's: Reduction, Reception, and Replacement". Chapter 7 of Maurice Schouten and Huib Looren de Jong (eds.), *The Matter of the Mind: Philosophical Essays on Psychology, Neuroscience, and Reduction*. Oxford, Blackwell, 2007.

Engesser, Kurt, Dov M. Gabbay and Daniel Lehmann (eds.). *Handbook of Quantum Logic and Quantum Structures: Quantum Logic*. Amsterdam, Elsevier, 2009.

European Food Safety Authority. “Final Review of the Séralini *et al.* (2012a) Publication on a 2-Year Rodent Feeding Study with Glyphosate Formulations and GM Maize NK603 as Published Online on 19 September 2012 in Food and Chemical Toxicology”. *EFSA Journal*, volume 10, number 11, 2012, pages 1-10 and Annex.
http://dx.doi.org/10.2903/j.efsa.2012.2986
Paper and Annex also available from
http://www.efsa.europa.eu/en/efsajournal/pub/2986.htm

European Journal of Social Theory. ISSN 1368-4310, eISSN 1461-7137.
http://est.sagepub.com/

Evans, Richard J. *In Defence of History*, new edition with afterword. London, Granta Books, 2000.

Fairweather, Abrol (ed.). *Virtue Epistemology Naturalized: Bridges Between Virtue Epistemology and Philosophy of Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-04672-3

Fallis, Don. “Attitudes Toward Epistemic Risk and the Value of Experiments”. *Studia Logica*, volume 86, number 2, 2007, pages 215-246.
http://dx.doi.org/10.1007/s11225-007-9060-y

Fallis, Don. “Intentional Gaps in Mathematical Proofs”. *Synthese*, volume 134, numbers 1-2, 2003, pages 45-69.
http://dx.doi.org/10.1023/A:1022131513275

Fanelli, Daniele. "How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data". *PLOS ONE*, volume 4, number 5, 2009, e5738, pages 1-11.
http://dx.doi.org/10.1371/journal.pone.0005738

Fantl, Jeremy, and Matthew McGrath. "Pragmatic Encroachment: It's Not Just About Knowledge". *Episteme*, volume 9, number 1, 2012, pages 27-42.
http://dx.doi.org/doi:10.1017/epi.2011.3

Farrell, Henry. *The Political Economy of Trust: Institutions, Interests, and Inter-Firm Cooperation in Italy and Germany*. Cambridge, Cambridge University Press, 2009.

Faye, Jan. "How Nature Makes Sense". Chapter 3 of Jan Faye, Paul Needham, Uwe Scheffler and Max Urchs (eds.), *Nature's Principles*. Dordrecht, Springer, 2005.

Faye, Jan. "Interpretation in the Natural Sciences". Chapter 9 of Mauricio Suárez, Mauro Dorato and Miklós Rédei (eds.), *EPSA Epistemology and Methodology of Science: Launch of the European Philosophy of Science Association, Volume 1*. Dordrecht, Springer, 2010.
http://dx.doi.org/10.1007/978-90-481-3263-8_9

Faye, Jan, Paul Needham, Uwe Scheffler and Max Urchs (eds.). *Nature's Principles*. Dordrecht, Springer, 2005.

Fearon, James D., and David D. Laitin. "Integrating Qualitative and Quantitative Methods". Chapter 33 of Janet M. Box-Steffensmeier, Henry E. Brady and David Collier (eds.), *The Oxford Handbook of Political Methodology*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/oxfordhb/9780199286546.003.0033

Feferman, Solomon. “Why the Programs for New Axioms Need to be Questioned”. Pages 401-413 of Solomon Feferman, Harvey M. Friedman, Penelope Maddy and John R. Steel, “Does Mathematics Need New Axioms?” *The Bulletin of Symbolic Logic*, volume 6, number 4, 2000, pages 401-446.
http://www.jstor.org/stable/420965

Fenton-Glynn, Luke. “*Ceteris Paribus* Laws and *Minutis Rectis* Laws”. Chapter 10 of *Philosophy of Science Association 24th Biennial Meeting*, preprint volume, version of 7 November 2014. Pittsburgh, PA, University of Pittsburgh for the Philosophy of Science Association, 2014.
http://philsci-archive.pitt.edu/10941/
(The papers from the meeting should be published in due course in *Philosophy of Science*, probably in two parts in December 2015 and December 2016.)

Ferguson, Jeffrey R. (ed.). *Designing Experimental Research in Archaeology: Examining Technology Through Production and Use*. Boulder, CO, University Press of Colorado, 2010.

Ferguson, Niall (ed.). *Virtual History: Alternatives and Counterfactuals*. London, Picador, 1997.

Ferrer i Cancho, Ramon, and Richard V. Solé. “Least Effort and the Origins of Scaling in Human Language”. *Proceedings of the National Academy of Sciences of the United States of America*, volume 100, number 3, 2003, pages 788-791.
http://dx.doi.org/10.1073/pnas.0335980100

Ferro, Marc. *Cinéma et Histoire*, nouvelle édition refondue. Paris, Éditions Gallimard, 1993.

Ferro, Marc. *Cinema and History*, translated by Naomi Greene. Detroit, MI, Wayne State University Press, 1988. (This is a translation of most of the content of the edition of *Cinéma et Histoire* that was published by Éditions Denoël/Gonthier, Paris, in 1977, with some revisions and additions.)

Feuchtwanger, Edgar. *Bismarck: A Political History*, second edition. London, Routledge, 2014.

Fikfak, Jurij, Frane Adam and Detlef Garz (eds.). *Qualitative Research: Different Perspectives, Emerging Trends.* Ljubljana, ZRC, 2004.

Fischer, David Hackett. *Historians' Fallacies: Toward a Logic of Historical Thought.* New York, NY, Harper and Row, 1970.

Fischer, John Martin. "Compatibilism". Chapter 2 of John Martin Fischer, Robert Kane, Derek Pereboom and Manuel Vargas, *Four Views on Free Will.* Oxford, Blackwell, 2007.

Florens, Jean-Pierre, Vêlayoudom Marimoutou and Anne Péguin-Feissolle. *Econometric Modeling and Inference*, translated by Josef Perktold and Marine Carrasco. Cambridge, Cambridge University Press, 2007.

Folbre, Nancy. *Valuing Children: Rethinking the Economics of the Family.* Cambridge, MA, Harvard University Press, 2008.

Folinsbee, Kaila E., David C. Evans, Jörg Fröbisch, Linda A. Tsuji and Daniel R. Brooks. “Quantitative Approaches to Phylogenetics”. Chapter 5 of Winfried Henke and Ian Tattersall (eds.), *Handbook of Paleoanthropology*, volume 1. Berlin, Springer, 2007. Updated online version of the chapter at
http://dx.doi.org/10.1007/978-3-642-27800-6_5-6

Forber, Patrick, and Eric Griffith. “Historical Reconstruction: Gaining Epistemic Access to the Deep Past”. *Philosophy and Theory in Biology*, volume 3, 2011, e203, pages 1-19.
http://dx.doi.org/10.3998/ptb.6959004.0003.003

Forster, Malcolm, and Elliott Sober. “How to Tell When Simpler, More Unified, or Less *Ad Hoc* Theories will Provide More Accurate Predictions”. *British Journal for the Philosophy of Science*, volume 45, number 1, 1994, pages 1-35.
http://dx.doi.org/10.1093/bjps/45.1.1

Fox, Peter, and James Hendler. “Changing the Equation on Scientific Data Visualization”. *Science*, volume 331, number 6018, 2011, pages 705-708.
http://dx.doi.org/10.1126/science.1197654

Franzese, Robert J. “Multicausality, Context-Conditionality, and Endogeneity”. Chapter 2 of Carles Boix and Susan C. Stokes (eds.), *The Oxford Handbook of Comparative Politics*. Oxford, Oxford University Press, 2007.
http://dx.doi.org/10.1093/oxfordhb/9780199566020.003.0002

Freeman, Alan, Andrew Kliman and Julian Wells (eds.). *The New Value Controversy and the Foundations of Economics*. Cheltenham, Edward Elgar, 2004.

French, Steven. “The Structure of Theories”. Chapter 28 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (chapter 25 of the first edition, 2008).

French, Steven, and Juha Saatsi (eds.). *The Continuum Companion to the Philosophy of Science*. London, Continuum, 2011.

Friedman, Daniel, and Shyam Sunder. *Experimental Methods: A Primer for Economists*. Cambridge, Cambridge University Press, 1994.
http://dx.doi.org/10.1017/CBO9781139174176

Friedman, Milton. “The Methodology of Positive Economics”. Chapter 1 of Friedman, *Essays in Positive Economics*. Chicago, IL, University of Chicago Press, 1953.

Friend, Michèle, Norma B. Goethe and Valentina S. Harizanov (eds.). *Induction, Algorithmic Learning Theory, and Philosophy*. Dordrecht, Springer, 2007.
http://dx.doi.org/10.1007/978-1-4020-6127-1

Frisch, Mathias. “No Place For Causes? Causal Skepticism in Physics”. *European Journal for Philosophy of Science*, volume 2, number 3, 2012, pages 313-336.
http://dx.doi.org/10.1007/s13194-011-0044-4

Frodeman, Robert, Julie Thompson Klein and Karl Mitcham (eds.). *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Frosh, Stephen, and Lisa Saville Young. “Psychoanalytic Approaches to Qualitative Psychology”. Chapter 7 of Carla Willig and Wendy Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Fu, Vincent Kang, Christopher Winship and Robert D. Mare. “Sample Selection Bias Models”. Chapter 18 of Melissa Hardy and Alan Bryman (eds.), *Handbook of Data Analysis*. London, Sage, 2004.

Fuhrmann, André. “Theories of Belief Change”. Chapter 56 of Sven Bernecker and Duncan Pritchard (eds.), *The Routledge Companion to Epistemology*. Abingdon, Routledge, 2011.

Gadamer, Hans-Georg. *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik* (Gesammelte Werke, Bände 1 und 2). Tübingen, J. C. B. Mohr (Paul Siebeck), 1990 (Band 1, 6. Auflage), 1993 (Band 2, 2. Auflage).

Gadamer, Hans-Georg. *Truth and Method*, second, revised edition, translation revised by Joel Weinsheimer and Donald G. Marshall. London, Sheed and Ward, 1989.

Galor, Oded. *Unified Growth Theory*. Princeton, NJ, Princeton University Press, 2011.

Ganesalingam, Mohan. *The Language of Mathematics: A Linguistic and Philosophical Investigation*. Dordrecht, Springer, 2013.

Ganesalingam, Mohan, and Timothy Gowers. “A Fully Automatic Problem Solver With Human-Style Output”, version 1, 17 September 2013.
http://arxiv.org/abs/1309.4501

García Landa, José Ángel. “Authorial Intention in Literary Hermeneutics: On Two American Theories”. *Miscelánea*, volume 12, 1991, pages 61-92.
http://ssrn.com/abstract=1737206

Geary, William. “Analysis of a Corrosion Under Insulation Failure in a Carbon Steel Refinery Hydrocarbon Line”. *Case Studies in Engineering Failure Analysis*, volume 1, number 4, 2013, pages 249-256.
http://dx.doi.org/10.1016/j.csefa.2013.09.001

Gellner, Ernest. “Concepts and Society”. Chapter 2 of Gellner, *Cause and Meaning in the Social Sciences: Selected Philosophical Themes, volume 1*, edited by I. C. Jarvie and Joseph Agassi. London, Routledge and Kegan Paul, 1973.

Gene Ontology. A project to standardize the terminology used to describe gene products.
http://geneontology.org/

George, Alexander L., and Andrew Bennett. *Case Studies and Theory Development in the Social Sciences*. Cambridge, MA, MIT Press, 2005.

Gerring, John. “Case Selection for Case-Study Analysis: Qualitative and Quantitative Techniques”. Chapter 28 of Janet M. Box-Steffensmeier, Henry E. Brady and David Collier (eds.), *The Oxford Handbook of Political Methodology*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/oxfordhb/9780199286546.003.0028

Gerring, John. “The Case Study: What it is and What it Does”. Chapter 4 of Carles Boix and Susan C. Stokes (eds.), *The Oxford Handbook of Comparative Politics*. Oxford, Oxford University Press, 2007.
http://dx.doi.org/10.1093/oxfordhb/9780199566020.003.0004

Giaquinto, Marcus. “Epistemology of Visual Thinking in Elementary Real Analysis”. British Journal for the Philosophy of Science, volume 45, number 3, 1994, pages 789-813.
http://dx.doi.org/10.1093/bjps/45.3.789

Giddens, Anthony. *The Constitution of Society: Outline of the Theory of Structuration*. Cambridge, Polity Press, 1984.

Giddens, Anthony, and Philip W. Sutton. *Sociology*, seventh edition. Cambridge, Polity Press, 2013.

Gigerenzer, Gerd, and Daniel G. Goldstein, “Reasoning the Fast and Frugal Way: Models of Bounded Rationality”. *Psychological Review*, volume 103, number 4, 1996, pages 650-669.
http://dx.doi.org/10.1037/0033-295X.103.4.650

Gijsbers, Victor. “Why Unification Is Neither Necessary Nor Sufficient for Explanation”. *Philosophy of Science*, volume 74, number 4, 2007, pages 481-500.
http://dx.doi.org/10.1086/524420

Gilbert, Nigel. *Agent-Based Models*. London, Sage, 2008.

Gilboa, Itzhak, Andrew Postlewaite, Larry Samuelson and David Schmeidler. “Economic Models as Analogies”. *The Economic Journal*, volume 124, number 578, 2014, pages F513-F533.
http://dx.doi.org/10.1111/ecoj.12128

Gillespie, Ronald J., and Paul L. A. Popelier. *Chemical Bonding and Molecular Geometry: From Lewis to Electron Densities*. Oxford, Oxford University Press, 2001.

Gintis, Herbert. *The Bounds of Reason: Game Theory and the Unification of the Behavioral Sciences*, revised edition. Princeton, NJ, Princeton University Press, 2014 (first edition 2009).

Gintis, Herbert, Samuel Bowles, Robert Boyd and Ernst Fehr (eds.). *Moral Sentiments and Material Interests: The Foundations of Cooperation in Economic Life*. Cambridge, MA, MIT Press, 2005.

Git. A distributed version control system.
http://git-scm.com/

Given, Lisa M. (ed.). *The Sage Encyclopedia of Qualitative Research Methods*, two volumes. London, Sage, 2008.
http://dx.doi.org/10.4135/9781412963909

Given-Wilson, Chris. *Chronicles: The Writing of History in Medieval England*. London, Hambledon and London, 2004.

Glaeser, Edward L. "Researcher Incentives and Empirical Methods". Chapter 13 of Andrew Caplin and Andrew Schotter (eds.), *The Foundations of Positive and Normative Economics: A Handbook*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/acprof:oso/9780195328318.003.0013

Glăveanu, Vlad Petre, and Koji Yamamoto (eds.). *Bridging History and Social Psychology*. Special issue of *Integrative Psychological and Behavioral Science*, volume 46, number 4, 2012.
http://link.springer.com/journal/12124/46/4/page/1

Glimcher, Paul W., and Ernst Fehr (eds.). *Neuroeconomics: Decision Making and the Brain*, second edition. London, Academic Press, 2014.

Glottopedia. A wiki for work in linguistics.
http://www.glottopedia.org/

Godfrey-Smith, Peter. "The Strategy of Model-Based Science". *Biology and Philosophy*, volume 21, number 5, 2006, pages 725-740.
http://dx.doi.org/10.1007/s10539-006-9054-6

Goertz, Gary. "Descriptive-Causal Generalizations: 'Empirical Laws' in the Social Sciences?" Chapter 5 of Harold Kincaid (ed.), *The Oxford Handbook of Philosophy of Social Science*. Oxford, Oxford University Press, 2012.
http://dx.doi.org/10.1093/oxfordhb/9780195392753.013.0005

Goldberg, Sanford C. *Relying on Others: An Essay in Epistemology*. Oxford, Oxford University Press, 2010.

Goldman, Alvin I. *Simulating Minds: The Philosophy, Psychology, and Neuroscience of Mindreading*. Oxford, Oxford University Press, 2006.

Goldman, Alvin I., and Moshe Shaked. "An Economic Model of Scientific Activity and Truth Acquisition". *Philosophical Studies*, volume 63, number 1, 1991, pages 31-55.
http://dx.doi.org/10.1007/BF00375996

Goldthorpe, John H. "'Cultural Capital': Some Critical Observations". *Sociologica*, 2007 number 2, 2007, pages 1-23.
http://www.sociologica.mulino.it/doi/10.2383/24755

Gomez, Basil, and John Paul Jones III (eds.). *Research Methods in Geography: A Critical Introduction*. Oxford, Blackwell, 2010.

González, Wenceslao J. (ed.). *Conceptual Revolutions: From Cognitive Science to Medicine*. A Coruña, Netbiblo, 2012.

Goodin, Robert E., and Charles Tilly (eds.). *The Oxford Handbook of Contextual Political Analysis*. Oxford, Oxford University Press, 2006.
http://dx.doi.org/10.1093/oxfordhb/9780199270439.001.0001

Goodwin, William. "Mechanisms and Chemical Reaction". Chapter 23 of Andrea I. Woody, Robin Findlay Hendry and Paul Needham (eds.), *Philosophy of Chemistry: Handbook of the Philosophy of Science, volume 6*. Amsterdam, North-Holland, 2012.
http://dx.doi.org/10.1016/B978-0-444-51675-6.50023-2

GoPubMed. A search engine for medical and related topics. http://www.gopubmed.org/web/gopubmed/ and http://help.gopubmed.com/

Gore, Jeff, Hyun Youk and Alexander van Oudenaarden. "Snowdrift Game Dynamics and Facultative Cheating in Yeast". *Nature*, volume 459, number 7244, 2009, pages 253-256.
http://dx.doi.org/10.1038/nature07921

Gowers, Timothy, and Michael Nielsen. "Massively Collaborative Mathematics". *Nature*, volume 461, number 7266, 2009, pages 879-881.
http://dx.doi.org/10.1038/461879a

Gray, Jim. "Jim Gray on eScience: A Transformed Scientific Method". Prefatory chapter of Tony Hey, Stewart Tansley and Kristin Tolle (eds.), *The Fourth Paradigm: Data-Intensive Scientific Discovery*. Redmond, WA, Microsoft Research, 2009.

Greco, John. *Achieving Knowledge: A Virtue-Theoretic Account of Epistemic Normativity.* Cambridge, Cambridge University Press, 2010.

Green, Sara. "When One Model is Not Enough: Combining Epistemic Tools in Systems Biology". *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, volume 44, number 2, 2013, pages 170-180.
http://dx.doi.org/10.1016/j.shpsc.2013.03.012

Greenhalgh, Trisha. *How to Read a Paper: The Basics of Evidence-Based Medicine*, fourth edition. Chichester, Wiley, 2010.

Greenland, Sander, and Babette Brumback. "An Overview of Relations among Causal Modelling Methods". *International Journal of Epidemiology*, volume 31, number 5, 2002, pages 1030-1037.
http://dx.doi.org/10.1093/ije/31.5.1030

Grieneisen, Michael L., and Minghua Zhang. "A Comprehensive Survey of Retracted Articles from the Scholarly Literature". *PLOS ONE*, volume 7, number 10, 2012, e44118, pages 1-15.
http://dx.doi.org/10.1371/journal.pone.0044118

Griffin, Emma. *A Short History of the British Industrial Revolution.* Basingstoke, Palgrave Macmillan, 2010.

Griffin, Larry J. "Narrative, Event-Structure Analysis, and Causal Interpretation in Historical Sociology". *American Journal of Sociology*, volume 98, number 5, 1993, pages 1094-1133.
http://www.jstor.org/stable/2781584

Griffiths, Paul E., and Karola Stotz. "Genes in the Postgenomic Era". *Theoretical Medicine and Bioethics*, volume 27, number 6, 2006, pages 499-521.
http://dx.doi.org/10.1007/s11017-006-9020-y

Griffiths, Paul E., and Karola Stotz. *Genetics and Philosophy: An Introduction*. Cambridge, Cambridge University Press, 2013.

Grimmer, Justin. "An Introduction to Bayesian Inference via Variational Approximations". *Political Analysis*, volume 19, number 1, 2011, pages 32-47.
http://dx.doi.org/10.1093/pan/mpq027

Grossman, Guy, and Delia Baldassarri. "The Impact of Elections on Cooperation: Evidence from a Lab-in-the-Field Experiment in Uganda". Chapter 8 of Gianluca Manzo (ed.), *Analytical Sociology: Actions and Networks*. Chichester, Wiley, 2014.

Grünbaum, Branko. "An Enduring Error". *Elemente der Mathematik*, volume 64, number 3, 2009, pages 89-101.
http://dx.doi.org/10.4171/EM/120
Also available as chapter 3 of Mircea Pitici (ed.), *The Best Writing on Mathematics 2010*. Princeton, NJ, Princeton University Press, 2011.

Grupen, Claus. "Physics of Particle Detection". *AIP Conference Proceedings*, volume 536, 2000, pages 3-34.
http://dx.doi.org/10.1063/1.1361756

Guala, Francesco. *The Methodology of Experimental Economics*. Cambridge, Cambridge University Press, 2005.

Guédon, Jean-Claude. “Sustaining the ‘Great Conversation’: The Future of Scholarly and Scientific Journals”. Chapter 3 of Bill Cope and Angus Phillips (eds.), *The Future of the Academic Journal*, second edition. Kidlington, Chandos, 2014.

Gul, Faruk, and Wolfgang Pesendorfer. “The Case for Mindless Economics”. Chapter 1 of Andrew Caplin and Andrew Schotter (eds.), *The Foundations of Positive and Normative Economics: A Handbook*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/acprof:oso/9780195328318.003.0001

Guo, Sujian. “Democratic Transition: A Critical Overview”. *Issues and Studies*, volume 35, number 4, 1999, pages 133-148.
http://www.airitilibrary.com/Publication/alDetailedMesh?DocID=10132511-199908-35-4-133-148-a

Gurova, Lilia. “Principles Versus Mechanisms in Cognitive Science”. Chapter 32 of Vassilios Karakostas and Dennis Dieks (eds.), *EPSA 11 Perspectives and Foundational Problems in Philosophy of Science. The European Philosophy of Science Association Proceedings, volume 2*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-3-319-01306-0_32

Hackeng, Tilman M., Kristin M. Seré and Jan Rosing. “Misconceptions About Protein-S Multimers”. *Blood*, volume 106, number 5, 2005, pages 1884-1885.
http://dx.doi.org/10.1182/blood-2004-12-4706

Hacking, Ian. “The Looping Effects of Human Kinds”. Chapter 12 of Dan Sperber, David Premack and Ann James Premack (eds.), *Causal Cognition: A Multidisciplinary Debate*. Oxford, Clarendon Press, 1995.
http://dx.doi.org/10.1093/acprof:oso/9780198524021.003.0012

Haefner, James W. *Modeling Biological Systems: Principles and Applications*, second edition. New York, NY, Springer, 2005.
http://dx.doi.org/10.1007/b106568

Haghshenas, Meysam, and Robert J. Klassen. “Analysis of Cracks Generated in the Spinning-Mandrel Teeth”. *Case Studies in Engineering Failure Analysis*, volume 2, number 2, 2014, pages 118-126.
http://dx.doi.org/10.1016/j.csefa.2014.06.004

Haines, Valerie A. “Evolutionary Explanations”. Chapter 6 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Hales, Steven D. (ed.). *A Companion to Relativism*. Oxford, Blackwell, 2011.

Halsted, David G. “Accuracy and Quality in Historical Representation: Wikipedia, Textbooks and the Investiture Controversy”. *Digital Medievalist*, volume 9, 2013.
http://www.digitalmedievalist.org/journal/9/halsted/

Hameroff, Stuart, and Roger Penrose. “Consciousness in the Universe: A Review of the ‘Orch OR’ Theory”. *Physics of Life Reviews*, volume 11, number 1, 2014, pages 39-78.
http://dx.doi.org/10.1016/j.plrev.2013.08.002

Hamming, Richard W. "The Unreasonable Effectiveness of Mathematics". *The American Mathematical Monthly*, volume 87, number 2, 1980, pages 81-90.
http://dx.doi.org/10.2307/2321982

Hampsher-Monk, Iain, Karin Tilmans and Frank van Vree (eds.). *History of Concepts: Comparative Perspectives*. Amsterdam, Amsterdam University Press, 1998.

Hanneke, David A., Shannon Fogwell and Gerald Gabrielse. "New Measurement of the Electron Magnetic Moment and the Fine Structure Constant". *Physical Review Letters*, volume 100, number 12, 2008, paper 120801, pages 1-4.
http://dx.doi.org/10.1103/PhysRevLett.100.120801

Hannon, Michael. "The Practical Origins of Epistemic Contextualism". *Erkenntnis*, volume 78, number 4, 2013, pages 899-919.
http://dx.doi.org/10.1007/s10670-012-9426-z

Hardwig, John. "The Role of Trust in Knowledge". *The Journal of Philosophy*, volume 88, number 12, 1991, pages 693-708.
http://www.jstor.org/stable/2027007

Hardy, Melissa, and Alan Bryman (eds.). *Handbook of Data Analysis*. London, Sage, 2004.

Harman, Gilbert, and Sanjeev Kulkarni. *Reliable Reasoning: Induction and Statistical Learning Theory*. Cambridge, MA, MIT Press, 2007.

Harré, Rom. *The Principles of Scientific Thinking*. London, Macmillan, 1970.

Hart, Herbert L. A., and Tony Honoré. *Causation in the Law*, second edition. Oxford, Clarendon Press, 1985.

Hartmann, Stephan. "Effective Field Theories, Reductionism and Scientific Explanation". *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, volume 32, number 2, 2001, pages 267-304.
http://dx.doi.org/10.1016/S1355-2198(01)00005-3

Hartmann, Stephan, Carlo Martini and Jan Sprenger. "Consensual Decision-Making Among Epistemic Peers". *Episteme*, volume 6, number 2, 2009, pages 110-129.
http://dx.doi.org/10.3366/E1742360009000598

Hausheer, Roger. "Three Major Originators of the Concept of *Verstehen*: Vico, Herder, Schleiermacher". Chapter 4 of Anthony O'Hear (ed.), *Verstehen and Humane Understanding*, Royal Institute of Philosophy Supplement 41. Cambridge, Cambridge University Press, 1996.

Hausman, Daniel M. *Causal Asymmetries*. Cambridge, Cambridge University Press, 1998.

Hawthorne, James. "Bayesian Confirmation Theory". Chapter 11 of Steven French and Juha Saatsi (eds.), *The Continuum Companion to the Philosophy of Science*. London, Continuum, 2011.

Healy, Kieran, and James Moody. "Data Visualization in Sociology". *Annual Review of Sociology*, volume 40, 2014, chapter 5, pages 105-128.
http://dx.doi.org/10.1146/annurev-soc-071312-145551

Heather, Peter. "Late Antiquity and the Early Medieval West". Chapter 5 of Michael Bentley (ed.), *Companion to Historiography*. London, Routledge, 1997.

Hedrick, Philip. "Genetic Drift". Section 4.1 of Jonathan B. Losos et al. (eds.), *The Princeton Guide to Evolution*. Princeton, NJ, Princeton University Press, 2014.

Hedström, Peter. *Dissecting the Social: On the Principles of Analytical Sociology*. Cambridge, Cambridge University Press, 2005.

Hedström, Peter, and Peter Bearman (eds.). *The Oxford Handbook of Analytical Sociology*. Oxford, Oxford University Press, 2009.

Hedström, Peter, and Peter Bearman. "What is Analytical Sociology All About? An Introductory Essay". Chapter 1 of Peter Hedström and Peter Bearman (eds.), *The Oxford Handbook of Analytical Sociology*. Oxford, Oxford University Press, 2009.

Hedström, Peter, and Lars Udehn. "Analytical Sociology and Theories of the Middle Range". Chapter 2 of Peter Hedström and Peter Bearman (eds.), *The Oxford Handbook of Analytical Sociology*. Oxford, Oxford University Press, 2009.

Hedström, Peter, and Petri Ylikoski. "Analytical Sociology and Rational-Choice Theory". Chapter 2 of Gianluca Manzo (ed.), *Analytical Sociology: Actions and Networks*. Chichester, Wiley, 2014.

Hegselmann, Rainer, and Ulrich Krause. "Deliberative Exchange, Truth, and Cognitive Division of Labour: A Low-Resolution Modeling Approach". *Episteme*, volume 6, number 2, 2009, pages 130-144.
http://dx.doi.org/10.3366/E1742360009000604

Hegselmann, Rainer, and Ulrich Krause. "Truth and Cognitive Division of Labour: First Steps towards a Computer Aided Social Epistemology". *Journal of Artificial Societies and Social Simulation*, volume 9, number 3, 2006, pages 1-28.
http://jasss.soc.surrey.ac.uk/9/3/10.html

Heidegger, Martin. *Sein und Zeit*, 17. Auflage. Tübingen, Max Niemeyer, 1993.

Heidegger, Martin. *Being and Time: A Translation of Sein und Zeit*, translated by Joan Stambaugh. Albany, NY, State University of New York Press, 1996.

Heidelberger, Michael. "Causal and Symbolic Understanding in Historical Epistemology". *Erkenntnis*, volume 75, number 3, 2011, pages 467-482.
http://dx.doi.org/10.1007/s10670-011-9343-6

Helbing, Dirk (ed.). *Social Self-Organization: Agent-Based Simulations and Experiments to Study Emergent Social Behavior*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-3-642-24004-1

Helfrich, Hede. "Beyond the Dilemma of Cross-Cultural Psychology: Resolving the Tension between Etic and Emic Approaches". *Culture and Psychology*, volume 5, number 2, 1999, pages 131-153.
http://dx.doi.org/10.1177/1354067X9952002

Henderson, David K. *Interpretation and Explanation in the Human Sciences*. Albany, NY, State University of New York Press, 1993.

Hendrikse, Jesse Love, Trish Elizabeth Parsons and Benedikt Hallgrímsson. "Evolvability as the Proper Focus of Evolutionary Developmental Biology". *Evolution and Development*, volume 9, number 4, 2007, pages 393-401.
http://dx.doi.org/10.1111/j.1525-142X.2007.00176.x

Hendry, Robin Findlay. "Philosophy of Chemistry". Chapter 15 of Steven French and Juha Saatsi (eds.), *The Continuum Companion to the Philosophy of Science*. London, Continuum, 2011.

Henke, Winfried, and Ian Tattersall (eds.). *Handbook of Paleoanthropology*, volume 1. Berlin, Springer, 2007. Updated online version at
http://dx.doi.org/10.1007/978-3-642-27800-6

Herder, Johann Gottfried von. *Philosophical Writings*, translated and edited by Michael N. Forster. Cambridge, Cambridge University Press, 2002.

Hettne, Kristina M., Harish Dharuri, Jun Zhao, Katherine Wolstencroft, Khalid Belhajjame, Stian Soiland-Reyes, Eleni Mina, Mark Thompson, Don Cruickshank, Lourdes Verdes-Montenegro, Julian Garrido, David de Roure, Oscar Corcho, Graham Klyne, Reinout van Schouwen, Peter A. C. 't Hoen, Sean Bechhofer, Carole Goble and Marco Roos. "Structuring Research Methods and Data with the Research Object Model: Genomics Workflows as a Case Study". *Journal of Biomedical Semantics*, volume 5, paper 41, 2014, pages 1-16.
http://dx.doi.org/10.1186/2041-1480-5-41

Hewstone, Miles, Wolfgang Stroebe and Klaus Jonas (eds.). *An Introduction to Social Psychology*, fifth edition. Chichester, BPS Blackwell, 2012.

Hexter, Jack H. "The Burden of Proof: Christopher Hill, *Change and Continuity in Seventeenth Century England*". *Times Literary Supplement*, number 3841, 24 October 1975, pages 1250-1252.

Hey, Tony, Stewart Tansley and Kristin Tolle (eds.). *The Fourth Paradigm: Data-Intensive Scientific Discovery*. Redmond, WA, Microsoft Research, 2009.

Heyes, Cecilia, and Uta Frith (eds.). "New Thinking: The Evolution of Human Cognition". *Philosophical Transactions of the Royal Society B*, volume 367, number 1599, 2012. http://rstb.royalsocietypublishing.org/content/367/1599.toc

Hiles, David, and Ivo Čermák. "Narrative Psychology". Chapter 9 of Carla Willig and Wendy Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Hill, Austin Bradford. "The Environment and Disease: Association or Causation?" *Proceedings of the Royal Society of Medicine*, volume 58, number 5, 1965, pages 295-300. http://www.ncbi.nlm.nih.gov/pmc/articles/PMC1898525/

Hirschfeld, Lawrence A. "On a Folk Theory of Society: Children, Evolution, and Mental Representations of Social Groups". *Personality and Social Psychology Review*, volume 5, number 2, 2001, pages 107-117. http://dx.doi.org/10.1207/S15327957PSPR0502_2

Hochachka, Wesley M., Rich Caruana, Daniel Fink, Art Munson, Mirek Riedewald, Daria Sorokina and Steve Kelling. "Data-Mining Discovery of Pattern and Process in Ecological Systems". *Journal of Wildlife Management*, volume 71, number 7, 2007, pages 2427-2437. http://dx.doi.org/10.2193/2006-503

Hodder, Ian, and Scott Hutson. *Reading the Past: Current Approaches to Interpretation in Archaeology*, third edition. Cambridge, Cambridge University Press, 2003.

Höfling, Felix, and Thomas Franosch. "Anomalous Transport in the Crowded World of Biological Cells". *Reports on Progress in Physics*, volume 76, number 4, 2013, paper 046602, pages 1-50.
http://dx.doi.org/10.1088/0034-4885/76/4/046602

Hoover, Kevin D., and Stephen J. Perez. "Three Attitudes Towards Data Mining". *Journal of Economic Methodology*, volume 7, number 2, 2000, pages 195-210.
http://dx.doi.org/10.1080/13501780050045083

Howard, Don. "Lost Wanderers in the Forest of Knowledge: Some Thoughts on the Discovery-Justification Distinction". Chapter 2 of Jutta Schickore and Friedrich Steinle (eds.), *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*. Dordrecht, Springer, 2006.
http://dx.doi.org/10.1007/1-4020-4251-5_2

Howell, David C. *Statistical Methods for Psychology*, seventh edition. Belmont, CA, Cengage Wadsworth, 2010.

Howell, Martha C., and Walter Prevenier. *From Reliable Sources: An Introduction to Historical Methods*. Ithaca, NY, Cornell University Press, 2001.

Hoyningen-Huene, Paul. *Systematicity: The Nature of Science*. Oxford, Oxford University Press, 2013.

Huber, Franz. "What is the Point of Confirmation?" *Philosophy of Science*, volume 72, number 5, 2005, pages 1146-1159.
http://dx.doi.org/10.1086/508961

Hulbert, Anthony J. "A Sceptics View: 'Kleiber's Law' or the '3/4 Rule' is neither a Law nor a Rule but Rather an Empirical Approximation". *Systems*, volume 2, number 2, 2014, pages 186-202.
http://dx.doi.org/10.3390/systems2020186

Hull, David L. *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*. Chicago, IL, University of Chicago Press, 1988.

Hunt, Lynn. "Against Presentism". *Perspectives on History*, online edition, volume 40, number 5, 2002.
http://www.historians.org/Perspectives/Issues/2002/0205/0205pre1.cfm

Hüttemann, Andreas. *What's Wrong With Microphysicalism?* London, Routledge, 2004.

Hutto, Daniel D. *Folk Psychological Narratives: The Sociocultural Basis of Understanding Reasons*. Cambridge, MA, MIT Press, 2008.

Illari, Phyllis McKay, and Jon Williamson. "What is a Mechanism? Thinking About Mechanisms *Across* the Sciences". *European Journal for Philosophy of Science*, volume 2, number 1, 2012, pages 119-135.
http://dx.doi.org/10.1007/s13194-011-0038-2

International Committee of Medical Journal Editors. *Recommendations for the Conduct, Reporting, Editing, and Publication of Scholarly Work in Medical Journals*, updated December 2014.
http://www.icmje.org/icmje-recommendations.pdf

International Union of Pure and Applied Chemistry. *IUPAC Compendium of Chemical Terminology - The Gold Book*. Online version derived from the second print edition compiled by Alan D. McNaught and Andrew Wilkinson, Oxford, Blackwell, 1997, and amended and updated by others.
http://goldbook.iupac.org

Ippoliti, Emiliano (ed.). *Heuristic Reasoning*. Dordrecht, Springer, 2015.
http://dx.doi.org/10.1007/978-3-319-09159-4

Iranzo, Valeriano. "Abduction and Inference to the Best Explanation". *Theoria*, volume 22, number 3, 2007, pages 339-346.
http://www.ehu.es/ojs/index.php/THEORIA/article/view/455

Iserloh, Erwin. *Luthers Thesenanschlag. Tatsache oder Legende?* Wiesbaden, Franz Steiner, 1962.

Ivanova, Milena. "Is There a Place for Epistemic Virtues in Theory Choice?" Chapter 13 of Abrol Fairweather (ed.), *Virtue Epistemology Naturalized: Bridges Between Virtue Epistemology and Philosophy of Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-04672-3_13

Ivanova, Milena, and Cedric Paternotte. "Theory Choice, Good Sense and Social Consensus". *Erkenntnis*, volume 78, number 5, 2013, pages 1109-1132.
http://dx.doi.org/10.1007/s10670-012-9390-7

Jablonka, Eva, and Gal Raz. "Transgenerational Epigenetic Inheritance: Prevalence, Mechanisms, and Implications for the Study of Heredity and Evolution". *The Quarterly Review of Biology*, volume 84, number 2, 2009, pages 131-176.
http://dx.doi.org/10.1086/598822

Jackson, Jonathan, Ben Bradford, Mike Hough, Andy Myhill, Paul Quinton and Tom R. Tyler. "Why do People Comply with the Law? Legitimacy and the Influence of Legal Institutions". *British Journal of Criminology*, volume 52, number 6, 2012, pages 1051-1071.
http://dx.doi.org/10.1093/bjc/azs032

Jackson, Robert. *Sovereignty: Evolution of an Idea*. Cambridge, Polity Press, 2007.

Jaffe, Arthur, and Frank Quinn. "'Theoretical Mathematics': Toward a Cultural Synthesis of Mathematics and Theoretical Physics". *Bulletin of the American Mathematical Society*, new series, volume 29, number 1, 1993, pages 1-13.
http://www.ams.org/journals/bull/1993-29-01/S0273-0979-1993-00413-0/

Jahnke, Hans Niels (ed.). *A History of Analysis*. Providence, RI, American Mathematical Society, 2003.

Jarrett, Mark. *The Congress of Vienna and its Legacy: War and Great Power Diplomacy After Napoleon*, new paperback edition. London, I. B. Tauris, 2014.

Jeauneau, Édouard. *Rethinking the School of Chartres*, translated by Claude Paul Desmarais. North York, Ontario, University of Toronto Press, 2009.

Jha, Alok. "Dan Schechtman: 'Linus Pauling Said I Was Talking Nonsense'". *The Observer*, 6 January 2013.
http://www.guardian.co.uk/science/2013/jan/06/dan-shechtman-nobel-prize-chemistry-interview

Joffe, Michael. "Causality and Evidence Discovery in Epidemiology". Chapter 11 of Dennis Dieks, Wenceslao J. González, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.), *Explanation, Prediction, and Confirmation*. Dordrecht, Springer, 2011.
http://dx.doi.org/10.1007/978-94-007-1180-8_11

Johansson, Maria U., Vincent Zoete, Olivier Michielin and Nicolas Guex. "Defining and Searching for Structural Motifs Using DeepView/Swiss-PdbViewer". *BMC Bioinformatics*, volume 13, paper 173, 2012, pages 1-10.
http://dx.doi.org/10.1186/1471-2105-13-173

John, Leslie K., George Loewenstein and Drazen Prelec. "Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling". *Psychological Science*, volume 23, number 5, 2012, pages 524-532.
http://dx.doi.org/10.1177/0956797611430953

Johnson, Gregory. "The Relationship Between Psychological Capacities and Neurobiological Activities". *European Journal of Philosophy of Science*, volume 2, number 3, 2012, pages 453-480.
http://dx.doi.org/10.1007/s13194-012-0053-y

Johnson, Ralph H. *Manifest Rationality: A Pragmatic Theory of Argument*. Mahwah, NJ, Lawrence Erlbaum Associates, 2000.

Jones, Andrew. *Archaeological Theory and Scientific Practice*. Cambridge, Cambridge University Press, 2001.

Journal of Artificial Societies and Social Simulation. eISSN 1460-7425.
http://jasss.soc.surrey.ac.uk/JASSS.html

Journal of Computational Chemistry. eISSN 1096-987X.
http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1096-987X

Journal of Evolutionary Biology. eISSN 1420-9101.
http://onlinelibrary.wiley.com/journal/10.1111/(ISSN)1420-9101

Journal of Experimental Political Science. ISSN 2052-2630, eISSN 2052-2649.
http://journals.cambridge.org/action/displayJournal?jid=XPS

Journal of Visualized Experiments.
http://www.jove.com

Jowett, Garth S., and Victoria J. O'Donnell. *Propaganda and Persuasion*, fifth edition. London, Sage, 2012.

Kaeuper, Richard W. *Chivalry and Violence in Medieval Europe*. Oxford, Oxford University Press, 1999.

Kaeuper, Richard W. (ed.). *Law, Governance, and Justice: New Views on Medieval Constitutionalism*. Leiden, Brill, 2013.

Kaiser, David. *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics*. Chicago, IL, University of Chicago Press, 2005.

Kant, Immanuel. *Kritik der reinen Vernunft* (*Critique of Pure Reason*). First edition (A), 1781; second edition (B), 1787.

Kaplan, Mark. “Decision Theory and Epistemology”. Chapter 15 of Paul K. Moser (ed.), *The Oxford Handbook of Epistemology*. Oxford, Oxford University Press, 2002.
http://dx.doi.org/10.1093/0195130057.003.0016

Karakostas, Vassilios, and Dennis Dieks (eds.). *EPSA 11 Perspectives and Foundational Problems in Philosophy of Science. The European Philosophy of Science Association Proceedings, volume 2*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-3-319-01306-0

Karban, Richard, Mikaela Huntzinger and Ian S. Pearse. *How To Do Ecology: A Concise Handbook*, second edition. Princeton, NJ, Princeton University Press, 2014.

Kawakatsu, Toshihiro. *Statistical Physics of Polymers: An Introduction*. Berlin, Springer, 2004.
http://dx.doi.org/10.1007/978-3-662-10024-0

Kellert, Stephen H., Helen E. Longino and C. Kenneth Waters (eds.). *Scientific Pluralism*. Minnesota Studies in the Philosophy of Science, 19. Minneapolis, MN, University of Minnesota Press, 2006.

Kelly, Kevin T. “How Simplicity Helps You Find the Truth Without Pointing At It”. Chapter 4 of Michèle Friend, Norma B. Goethe and Valentina S. Harizanov (eds.), *Induction, Algorithmic Learning Theory, and Philosophy*. Dordrecht, Springer, 2007.
http://dx.doi.org/10.1007/978-1-4020-6127-1_4

Kelly, Kevin T. “Simplicity, Truth, and Probability”. Chapter 31 of Prasanta S. Bandyopadhyay and Malcolm R. Forster (eds.), *Philosophy of Statistics: Handbook of the Philosophy of Science, volume 7*. Amsterdam, North-Holland, 2011.
http://www.sciencedirect.com/science/article/pii/B9780444518620500319

Kelly, Kevin T. “Simplicity, Truth, and the Unending Game of Science”. Chapter 11 of Stefan Bold, Benedikt Löwe, Thoralf Räsch and Johan van Benthem (eds.), *Foundations of the Formal Sciences V: Infinite Games*. London, College Publications, 2007.

Kelly, Kevin T., and Conor Mayo-Wilson. “Ockham Efficiency Theorem for Stochastic Empirical Methods”. *Journal of Philosophical Logic*, volume 39, number 6, 2010, pages 679-712.
http://dx.doi.org/10.1007/s10992-010-9145-3

Kelp, Christoph, and Igor Douven. “Sustaining a Rational Disagreement”. Chapter 10 of Henk W. de Regt, Stephan Hartmann and Samir Okasha (eds.), *EPSA Philosophy of Science: Amsterdam 2009. The European Philosophy of Science Association Proceedings, volume 1*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-2404-4_10

Kerr, Eric, and Axel Gelfert. “The ‘Extendedness’ of Scientific Evidence”. *Philosophical Issues*, volume 24, number 1, 2014, pages 253-281.
http://dx.doi.org/10.1111/phis.12033

Kessler, Amalia D. “Power, Not Progress: An Alternative Reading of L’Hôpital’s Legal Reforms”. *Law and History Review*, volume 28, number 3, 2010, pages 837-842.
http://dx.doi.org/10.1017/S0738248010000659

Kettunen, Marianne, and Patrick ten Brink. *Value of Biodiversity: Documenting EU Examples Where Biodiversity Loss has led to the Loss of Ecosystem Services, Final Report for the European Commission*. Brussels, Institute for European Environmental Policy, 2006.
http://ec.europa.eu/environment/enveco/pdf/value_biodiversity.pdf

Khalidi, Muhammad Ali. *Natural Categories and Human Kinds: Classification in the Natural and Social Sciences*. Cambridge, Cambridge University Press, 2013.

Kincaid, Harold. "Formal Rationality and Its Pernicious Effects on the Social Sciences". *Philosophy of the Social Sciences*, volume 30, number 1, 2000, pages 67-88.
http://dx.doi.org/10.1177/004839310003000104

Kincaid, Harold. "Functional Explanation and Evolutionary Social Science". Chapter 5 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Kincaid, Harold. "Mechanisms, Causal Modeling, and the Limitations of Traditional Multiple Regression". Chapter 3 of Harold Kincaid (ed.), *The Oxford Handbook of Philosophy of Social Science*. Oxford, Oxford University Press, 2012.
http://dx.doi.org/10.1093/oxfordhb/9780195392753.013.0003

Kincaid, Harold (ed.). *The Oxford Handbook of Philosophy of Social Science*. Oxford, Oxford University Press, 2012.
http://dx.doi.org/10.1093/oxfordhb/9780195392753.001.0001

Kincaid, Harold, John Dupré and Alison Wylie (eds.). *Value-Free Science? Ideals and Illusions*. Oxford, Oxford University Press, 2007.

Kincaid, Harold, and Don Ross (eds.). *The Oxford Handbook of Philosophy of Economics*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780195189254.001.0001

King, Gary, Robert O. Keohane and Sidney Verba. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton, NJ, Princeton University Press, 1994.

King's College London Centre for e-Research website.
http://www.kcl.ac.uk/innovation/groups/cerch/index.aspx

Kitcher, Philip. "Explanatory Unification and the Causal Structure of the World". Chapter 9 of Philip Kitcher and Wesley C. Salmon (eds.), *Scientific Explanation*. Minnesota Studies in the Philosophy of Science, 13. Minneapolis, MN, University of Minnesota Press, 1989.

Kitcher, Philip, and Wesley C. Salmon (eds.). *Scientific Explanation*. Minnesota Studies in the Philosophy of Science, volume 13. Minneapolis, MN, University of Minnesota Press, 1989.

Klein, Julie Thompson, and Richard Parncutt. "Art and Music Research". Chapter 9 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Kleinberg, Samantha. *Causality, Probability, and Time*. Cambridge, Cambridge University Press, 2013.

Kleiner, Israel. “From Fermat to Wiles: Fermat’s Last Theorem Becomes a Theorem”. *Elemente der Mathematik*, volume 55, number 1, 2000, pages 19-37.
http://dx.doi.org/10.1007/PL00000079

Kline, Rex B. *Principles and Practice of Structural Equation Modeling*, third edition. New York, NY, Guilford Press, 2011.

Kloos, Peter. “Multiple Images of Ethnic Reality: Beyond Disagreement?” Chapter 4 of Anton van Harskamp (ed.), *Conflicts in Social Science*. London, Routledge, 1996.

Knobe, Joshua. “Philosophers Are Doing Something Different Now: Quantitative Data”. *Cognition*, volume 135, 2015, pages 36-38, with supplementary data available in the online version.
http://dx.doi.org/10.1016/j.cognition.2014.11.011

Knoop, Todd A. *Recessions and Depressions: Understanding Business Cycles*, second edition. Santa Barbara, CA, Praeger, 2010.

Kocka, Jürgen. *Industrial Culture and Bourgeois Society: Business, Labor, and Bureaucracy in Modern Germany*. New York, NY, Berghahn Books, 1999.

Kogut, Bruce. “Qualitative Comparative Analysis of Social Science Data”. Chapter 6 of Glenn Morgan, John L. Campbell, Colin Crouch, Ove Kaj Pedersen and Richard Whitley (eds.), *The Oxford Handbook of Comparative Institutional Analysis*. Oxford, Oxford University Press, 2010.
http://dx.doi.org/10.1093/oxfordhb/9780199233762.003.0007

Kölbel, Max. "Vagueness as Semantic". Chapter 17 of Richard Dietz and Sebastiano Moruzzi (eds.), *Cuts and Clouds: Vagueness, its Nature and its Logic*. Oxford, Oxford University Press, 2010.

Koop, Thomas, and Ulrich Pöschl. "Systems: An Open, Two-Stage Peer-Review Journal". *Nature: Peer Review Debate*, published online, 2006.
http://www.nature.com/nature/peerreview/debate/nature04988.html

Koselleck, Reinhart. "*Begriffsgeschichte* and Social History". Chapter 5 of Koselleck, *Futures Past: On the Semantics of Historical Time*, translated and with an introduction by Keith Tribe (revised version of the Cambridge, MA, MIT Press, 1985 edition). New York, NY, Columbia University Press, 2004.

Koselleck, Reinhart. "Historia Magistra Vitae: The Dissolution of the Topos into the Perspective of a Modernized Historical Process". Chapter 2 of Koselleck, *Futures Past: On the Semantics of Historical Time*, translated and with an introduction by Keith Tribe (revised version of the Cambridge, MA, MIT Press, 1985 edition). New York, NY, Columbia University Press, 2004.

Koselleck, Reinhart. "Historical Criteria of the Modern Concept of Revolution". Chapter 3 of Koselleck, *Futures Past: On the Semantics of Historical Time*, translated and with an introduction by Keith Tribe (revised version of the Cambridge, MA, MIT Press, 1985 edition). New York, NY, Columbia University Press, 2004.

Koselleck, Reinhart. “Representation, Event, and Structure”. Chapter 7 of Koselleck, *Futures Past: On the Semantics of Historical Time*, translated and with an introduction by Keith Tribe (revised version of the Cambridge, MA, MIT Press, 1985 edition). New York, NY, Columbia University Press, 2004.

Kosso, Peter. *Knowing the Past: Philosophical Issues of History and Archaeology*. Amherst, NY, Humanity Books, 2001.

Kourany, Janet A. “Replacing the Ideal of Value-Free Science”. Chapter 4 of Martin Carrier, Don Howard and Janet A. Kourany (eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*. Pittsburgh, PA, University of Pittsburgh Press, 2008.

Krabbe, Erik C. W. “Arguments, Proofs, and Dialogues”. Chapter 3 of Andrew Aberdein and Ian J. Dove (eds.), *The Argument of Mathematics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-6534-4_3

Kramer, Lloyd, and Sarah Maza (eds.). *A Companion to Western Historical Thought*. Oxford, Blackwell, 2002.

Kraus, Jody S. “Transparency and Determinacy in Common Law Adjudication: A Philosophical Defense of Explanatory Economic Analysis”. *Virginia Law Review*, volume 93, number 2, 2007, pages 287-359.
http://www.virginialawreview.org/sites/virginialawreview.org/files/287_0.pdf

Kroeber, Clifton B. “Theory and History of Revolution”. *Journal of World History*, volume 7, number 1, 1996, pages 21-40.
http://dx.doi.org/10.1353/jwh.2005.0056

Krugman, Paul. "The Facebooking of Economics". *New York Times*, 17 December 2013.
http://krugman.blogs.nytimes.com/2013/12/17/the-facebooking-of-economics/

Kuhn, Thomas S. "Logic of Discovery or Psychology of Research?" Chapter 1 of Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, volume 4*. Cambridge, Cambridge University Press, 1970.

Kuhn, Thomas S. *The Structure of Scientific Revolutions*, third edition. Chicago, IL, University of Chicago Press, 1996.

Kuipers, Theo A. F. *Structures in Science: Heuristic Patterns Based on Cognitive Structures*. Dordrecht, Kluwer Academic Publishers, 2001.
http://dx.doi.org/10.1007/978-94-015-9739-5

Kunz, Werner. *Do Species Exist? Principles of Taxonomic Classification*. Weinheim, Wiley-VCH, 2012.

Kuorikoski, Jaakko. "Contrastive Statistical Explanation and Causal Heterogeneity". *European Journal for Philosophy of Science*, volume 2, number 3, 2012, pages 435-452.
http://dx.doi.org/10.1007/s13194-012-0050-1

Kuorikoski, Jaakko. "Two Concepts of Mechanism: Componential Causal System and Abstract Form of Interaction". *International Studies in the Philosophy of Science*, volume 23, number 2, 2009, pages 143-160.
http://dx.doi.org/10.1080/02698590903006875

Kuorikoski, Jaakko, Aki Lehtinen and Caterina Marchionni. "Economic Modelling as Robustness Analysis". *British Journal for the Philosophy of Science*, volume 61, number 3, 2010, pages 541-567.
http://dx.doi.org/10.1093/bjps/axp049

Kuorikoski, Jaakko, and Petri Ylikoski. "How Organization Explains". Chapter 6 of Vassilios Karakostas and Dennis Dieks (eds.), *EPSA 11 Perspectives and Foundational Problems in Philosophy of Science. The European Philosophy of Science Association Proceedings, volume 2*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-3-319-01306-0_6

Kurz, Sascha, and Jörg Rambau. "On the Hegselmann-Krause Conjecture in Opinion Dynamics". *Journal of Difference Equations and Applications*, volume 17, number 6, 2011, pages 859-876.
http://dx.doi.org/10.1080/10236190903443129

Kutach, Douglas. *Causation and its Basis in Fundamental Physics*. Oxford, Oxford University Press, 2013.
http://dx.doi.org/10.1093/acprof:oso/9780199936205.001.0001

Kvanvig, Jonathan L. *The Value of Knowledge and the Pursuit of Understanding*. Cambridge, Cambridge University Press, 2003.

Lackey, Jennifer. *Learning from Words: Testimony as a Source of Knowledge*. Oxford, Oxford University Press, 2008.

Lahroodi, Reza. "Collective Epistemic Virtues". *Social Epistemology*, volume 21, number 3, 2007, pages 281-297.
http://dx.doi.org/10.1080/02691720701674122

Lakatos, Imre, and Alan Musgrave (eds.). *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, volume 4*. Cambridge, Cambridge University Press, 1970.

Lakhani, Karim R., Kevin J Boudreau, Po-Ru Loh, Lars Backstrom, Carliss Baldwin, Eric Lonstein, Mike Lydon, Alan MacCormack, Ramy A. Arnaout and Eva C. Guinan. "Prize-Based Contests Can Provide Solutions to Computational Biology Problems". *Nature Biotechnology*, volume 31, number 2, 2013, pages 108-111.
http://dx.doi.org/10.1038/nbt.2495

Lakoff, George, and Rafael E. Núñez. *Where Mathematics Comes From: How the Embodied Mind Brings Mathematics into Being*. New York, NY, Basic Books, 2000.

Laland, Kevin N., John Odling-Smee, William Hoppitt and Tobias Uller. "More on How and Why: Cause and Effect in Biology Revisited". *Biology and Philosophy*, volume 28, number 5, 2013, pages 719-745.
http://dx.doi.org/10.1007/s10539-012-9335-1

Landreth, Anthony. "The Emerging Theory of Motivation". Chapter 15 of John Bickle (ed.), *The Oxford Handbook of Philosophy and Neuroscience*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780195304787.003.0016

Landry, Pierre F., and Mingming Shen. "Reaching Migrants in Survey Research: The Use of the Global Positioning System to Reduce Coverage Bias in China". *Political Analysis*, volume 13, number 1, 2005, pages 1-22.
http://dx.doi.org/10.1093/pan/mpi001

Lane Fox, Robin. *Alexander the Great*, updated edition. London, Penguin, 2004.

Lange, Marc. “Laws and Theories”. Chapter 26 of Sahotra Sarkar and Anya Plutynski (eds.), *A Companion to the Philosophy of Biology*. Oxford, Blackwell, 2008.

Larson, Allan. “Concepts in Character Macroevolution: Adaptation, Homology, and Evolvability”. Section 2.6 of Jonathan B. Losos et al. (eds.), *The Princeton Guide to Evolution*. Princeton, NJ, Princeton University Press, 2014.

Larvor, Brendan. “What Philosophy of Mathematical Practice Can Teach Argumentation Theory About Diagrams and Pictures”. Chapter 13 of Andrew Aberdein and Ian J. Dove (eds.), *The Argument of Mathematics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-6534-4_13

Laubichler, Manfred D., and Gerd B. Müller (eds.). *Modeling Biology: Structures, Behaviors, Evolution*. Cambridge, MA, MIT Press, 2007.

Laubichler, Manfred D., and Gerd B. Müller. “Models in Theoretical Biology”. Chapter 1 of Manfred D. Laubichler and Gerd B. Müller (eds.), *Modeling Biology: Structures, Behaviors, Evolution*. Cambridge, MA, MIT Press, 2007.

Laudan, Larry. *Beyond Positivism and Relativism: Theory, Method, and Evidence*. Boulder, CO, Westview Press, 1996.

Lawvere, F. William, and Robert Rosebrugh. *Sets for Mathematics*. Cambridge, Cambridge University Press, 2003.

Le, Thuc Duy, Lin Liu, Anna Tsykin, Gregory J. Goodall, Bing Liu, Bing-Yu Sun and Jiuyong Li. "Inferring microRNA-mRNA Causal Regulatory Relationships from Expression Data". *Bioinformatics*, volume 29, number 6, 2013, pages 765-771.
http://dx.doi.org/10.1093/bioinformatics/btt048

Le Bihan, Soazig. "Defending the Semantic View: What It Takes". *European Journal for Philosophy of Science*, volume 2, number 3, 2012, pages 249-274.
http://dx.doi.org/10.1007/s13194-011-0026-6

Lear, Jonathan. "What is a Crisis of Intelligibility?" Chapter 8 of Geoffrey Scarre and Robin Coningham (eds.), *Appropriating the Past: Philosophical Perspectives on the Practice of Archaeology*. Cambridge, Cambridge University Press, 2013.
http://dx.doi.org/10.1017/CBO9781139026932.010

Lefevere, Merel, and Eric Schliesser. "Private Epistemic Virtue, Public Vices: Moral Responsibility in the Policy Sciences". Chapter 14 of Carlo Martini and Marcel Boumans (eds.), *Experts and Consensus in Social Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-08551-7_14

Leitgeb, Hannes. "On Formal and Informal Provability". Chapter 12 of Otávio Bueno and Øystein Linnebo (eds.), *New Waves in Philosophy of Mathematics*. Basingstoke, Palgrave Macmillan, 2009.

Lendon, Jon E. *Empire of Honour: The Art of Government in the Roman World*. Oxford, Clarendon Press, 1997.

Leonelli, Sabina. "What is a Model? Combining Theoretical and Material Models to Develop Intelligible Theories". Chapter 2 of Manfred D. Laubichler and Gerd B. Müller (eds.), *Modeling Biology: Structures, Behaviors, Evolution*. Cambridge, MA, MIT Press, 2007.

Levy, Jack S. "Case Studies: Types, Designs, and Logics of Inference". *Conflict Management and Peace Science*, volume 25, number 1, 2008, pages 1-18.
http://dx.doi.org/10.1080/07388940701860318

Levy, Jack S. "Counterfactuals and Case Studies". Chapter 27 of Janet M. Box-Steffensmeier, Henry E. Brady and David Collier (eds.), *The Oxford Handbook of Political Methodology*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/oxfordhb/9780199286546.003.0027

Lewellen, Jonathan, and Stefan Nagel. "The Conditional CAPM Does Not Explain Asset-Pricing Anomalies". *Journal of Financial Economics*, volume 82, number 2, 2006, pages 289-314.
http://dx.doi.org/10.1016/j.jfineco.2005.05.012

Li, Yarui, Joshua D. Woodard and David J. Leatham. "Causality among Foreign Direct Investment and Economic Growth: A Directed Acyclic Graph Approach". *Journal of Agricultural and Applied Economics*, volume 45, number 4, 2013. pages 617-637.
http://purl.umn.edu/157392

Lidin, Sven. *The Discovery of Quasicrystals: Scientific Background on the Nobel Prize in Chemistry 2011*. Stockholm, Royal Swedish Academy of Sciences, 2011.
http://www.nobelprize.org/nobel_prizes/chemistry/laureates/2011/advanced-chemistryprize2011.pdf

Lipsey, Richard, and Alec Chrystal. *Economics*, thirteenth edition. Oxford, Oxford University Press, 2015.

Lipton, Peter. *Inference to the Best Explanation*, second edition. London, Routledge, 2004.

Little, Daniel. "Levels of the Social". Chapter 8 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Little, Todd D. (ed.). *The Oxford Handbook of Quantitative Methods: Volume 1, Foundations*. (Alternative title: *The Oxford Handbook of Quantitative Methods in Psychology: Volume 1, Foundations*.) Oxford, Oxford University Press, 2013.
http://dx.doi.org/10.1093/oxfordhb/9780199934874.001.0001

Little, Todd D. (ed.). *The Oxford Handbook of Quantitative Methods: Volume 2, Statistical Analysis*. (Alternative title: *The Oxford Handbook of Quantitative Methods in Psychology: Volume 2, Statistical Analysis*.) Oxford, Oxford University Press, 2013.
http://dx.doi.org/10.1093/oxfordhb/9780199934898.001.0001

Littlewood, John E. *Littlewood's Miscellany*, edited by Béla Bollobás. Cambridge, Cambridge University Press, 1986.

Lloyd, Geoffrey E. R. *Being, Humanity, and Understanding: Studies in Ancient and Modern Societies*. Oxford, Oxford University Press, 2012.
http://dx.doi.org/10.1093/acprof:oso/9780199654727.001.0001

Longino, Helen E. "Evidence in the Sciences of Behavior". Chapter 12 of Peter Achinstein (ed.), *Scientific Evidence: Philosophical Theories and Applications*. Baltimore, MD, Johns Hopkins University Press, 2005.

Longino, Helen E. *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton, NJ, Princeton University Press, 1990.

Longino, Helen E. *Studying Human Behavior: How Scientists Investigate Aggression and Sexuality*. Chicago, IL, University of Chicago Press, 2013.

Longino, Helen E. *The Fate of Knowledge*. Princeton, NJ, Princeton University Press, 2002.

Longino, Helen E. "Theoretical Pluralism and the Scientific Study of Behavior". Chapter 6 of Stephen H. Kellert, Helen E. Longino and C. Kenneth Waters (eds.), *Scientific Pluralism*. Minnesota Studies in the Philosophy of Science, 19. Minneapolis, MN, University of Minnesota Press, 2006.

Longino, Helen E. "What's So Great about an Objective Concept of Evidence?" Chapter 10 of Gregory J. Morgan (ed.), *Philosophy of Science Matters: The Philosophy of Peter Achinstein*. Oxford, Oxford University Press, 2011.

Losos, Jonathan B., David A. Baum, Douglas J. Futuyma, Hopi E. Hoekstra, Richard E. Lenski, Allen J. Moore, Catherine L. Peichel, Dolph Schluter and Michael C. Whitlock (eds.). *The Princeton Guide to Evolution*. Princeton, NJ, Princeton University Press, 2014.

Love, Alan C. (ed.). *Conceptual Change in Biology: Scientific and Philosophical Perspectives on Evolution and Development*. Dordrecht, Springer, 2015.
http://dx.doi.org/10.1007/978-94-017-9412-1

Lukes, Steven. "The Problem of Apparently Irrational Beliefs". Chapter 16 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Lustick, Ian S. "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias". *American Political Science Review*, volume 90, number 3, 1996, pages 605-618.
http://dx.doi.org/10.2307/2082612

Lützen, Jesper. "The Foundation of Analysis in the 19th Century". Chapter 6 of Hans Niels Jahnke (ed.), *A History of Analysis*. Providence, RI, American Mathematical Society, 2003.

Lykken, Joseph, and Maria Spiropulu. "Supersymmetry and the Crisis in Physics". *Scientific American*, volume 310, number 5, 2014, pages 34-39.
http://dx.doi.org/10.1038/scientificamerican0514-34

Lyth, David H., and Andrew R. Liddle. *The Primordial Density Perturbation: Cosmology, Inflation and the Origin of Structure*. Cambridge, Cambridge University Press, 2009.

Mac Lane, Saunders. "Despite Physicists, Proof is Essential in Mathematics". *Synthese*, volume 111, number 2, 1997, pages 147-154.
http://dx.doi.org/10.1023/A:1004918402670

MacIntyre, Alasdair. *After Virtue: A Study in Moral Theory*, third edition. London, Duckworth, 2007.

Mackie, John L. *The Cement of the Universe: A Study of Causation*, corrected edition. Oxford, Clarendon Press, 1980.

Maclean, Michael J. "Johann Gustav Droysen and the Development of Historical Hermeneutics". *History and Theory*, volume 21, number 3, 1982, pages 347-365.
http://www.jstor.org/stable/2505095

Madden, James J. "Book Review: Where Mathematics Comes From: How the Embodied Mind Brings Mathematics into Being". *Notices of the American Mathematical Society*, volume 48, number 10, 2001, pages 1182-1188.

Maddox, John, James Randi and Walter W. Stewart. "'High-Dilution' Experiments a Delusion".
Nature, volume 334, number 6180, 1988, pages 287-290.
http://dx.doi.org/10.1038/334287a0

Maddy, Penelope. *Defending the Axioms: On the Philosophical Foundations of Set Theory*. Oxford, Oxford University Press, 2011.
http://dx.doi.org/10.1093/acprof:oso/9780199596188.001.0001

Magnus, P. D. "On Trusting *Wikipedia*". *Episteme*, volume 6, number 1, 2009, pages 74-90.
http://journals.cambridge.org/action/displayAbstract?fromPage=online&aid=8356180&fileId=S1742360000001210

Magnusson, Lars, and Jan Ottosson (eds.). *The Evolution of Path Dependence*. Cheltenham, Edward Elgar, 2009.

Magnússon, Sigurður Gylfi, and István M. Szijártó. *What is Microhistory? Theory and Practice*. Abingdon, Routledge, 2013.

Mahoney, James. “Path Dependence in Historical Sociology”. *Theory and Society*, volume 29, number 4, 2000, pages 507-548.
http://dx.doi.org/10.1023/A:1007113830879

Mahoney, James. “The Logic of Process Tracing Tests in the Social Sciences”. *Sociological Methods and Research*, volume 41, number 4, 2012, pages 570-597.
http://dx.doi.org/10.1177/0049124112437709

Mahoney, James, Erin Kimball and Kendra L. Koivu. “The Logic of Historical Explanation in the Social Sciences”. *Comparative Political Studies*, volume 42, number 1, 2009, pages 114-146.
http://dx.doi.org/10.1177/0010414008325433

Mahoney, James, and Dietrich Rueschemeyer (eds.). *Comparative Historical Analysis in the Social Sciences*. Cambridge, Cambridge University Press, 2003.

Mahoney, James, and Daniel Schensul. “Historical Context and Path Dependence”. Chapter 24 of Robert E. Goodin and Charles Tilly (eds.), *The Oxford Handbook of Contextual Political Analysis*. Oxford, Oxford University Press, 2006.
http://dx.doi.org/10.1093/oxfordhb/9780199270439.003.0024

Mäki, Uskali. “MISSing the World. Models as Isolations and Credible Surrogate Systems”. *Erkenntnis*, volume 70, number 1, 2009, pages 29-43.
http://dx.doi.org/10.1007/s10670-008-9135-9

Malle, Bertram F. *How the Mind Explains Behavior: Folk Explanations, Meaning, and Social Interaction*. Cambridge, MA, MIT Press, 2004.

Malmberg, Bo, Thomas Lindh and Max Halvarsson. *Productivity Consequences of Workforce Ageing – Stagnation or a Horndal effect?* Stockholm, Institute for Futures Studies, Working Paper 2005 number 17, 2005.
http://www.iffs.se/en/publication/productivity-consequences-of-workforce-ageing-stagnation-or-a-horndal-effect-3/

Mancosu, Paolo (ed.). *The Philosophy of Mathematical Practice*. Oxford, Oxford University Press, 2008.

Mandelbaum, Maurice. "A Note on History as Narrative". Chapter 3 of Geoffrey Roberts (ed.), *The History and Narrative Reader*. London, Routledge, 2001.

Manicas, Peter T. *A Realist Philosophy of Social Science: Explanation and Understanding*. Cambridge, Cambridge University Press, 2006.

Mann, Kenneth G. "Analyses of Protein S Function". *Blood*, volume 106, number 5, 2005, page 1884.
http://dx.doi.org/10.1182/blood-2004-12-4706

Mantzavinos, Chrysostomos (ed.). *Philosophy of the Social Sciences: Philosophical Theory and Scientific Practice*. Cambridge, Cambridge University Press, 2009.

Manzo, Gianluca (ed.). *Analytical Sociology: Actions and Networks*. Chichester, Wiley, 2014.

Marshall, Peter. *Reformation England 1480-1642*, second edition. London, Bloomsbury, 2012.

Martin, Debra L., Ryan P. Harrod and Ventura R. Pérez. *Bioarchaeology: An Integrated Approach to Working With Human Remains*. New York, NY, Springer, 2013.
http://dx.doi.org/10.1007/978-1-4614-6378-8

Martini, Carlo. “Consensus Formation in Networked Groups”. Chapter 18 of Henk W. de Regt, Stephan Hartmann and Samir Okasha (eds.), *EPSA Philosophy of Science: Amsterdam 2009. The European Philosophy of Science Association Proceedings, volume 1*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-2404-4_18

Martini, Carlo, and Marcel Boumans (eds.). *Experts and Consensus in Social Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-08551-7

Martini, Carlo, Jan Sprenger and Mark Colyvan. “Resolving Disagreement Through Mutual Respect”. *Erkenntnis*, volume 78, number 4, 2013, pages 881-898.
http://dx.doi.org/10.1007/s10670-012-9381-8

Martinson, Brian C., Melissa S. Anderson and Raymond de Vries. “Scientists Behaving Badly”. *Nature*, volume 435, number 7043, 2005, pages 737-738.
http://dx.doi.org/10.1038/435737a

Maryland Institute for Technology in the Humanities website.
http://mith.umd.edu/

Massey, Doreen B. *For Space*. London, Sage, 2005.

Mathematical Social Sciences. Journal. ISSN 0165-4896.
http://www.journals.elsevier.com/mathematical-social-sciences/

Matthews, Bob, and Liz Ross. *Research Methods: A Practical Guide for the Social Sciences*. Harlow, Pearson Education, 2010.

Matthewson, John, and Brett Calcott. "Mechanistic Models of Population-Level Phenomena". *Biology and Philosophy*, volume 26, number 5, 2011, pages 737-756.
http://dx.doi.org/10.1007/s10539-011-9277-z

Mauskopf, Seymour, and Tad Schmaltz (eds.). *Integrating History and Philosophy of Science: Problems and Prospects*. Dordrecht, Springer, 2012.

Maxwell, Scott E., and Harold D. Delaney. *Designing Experiments and Analyzing Data: A Model Comparison Perspective*, second edition. Mahwah, NJ, Lawrence Erlbaum Associates, 2004.

May, Larry, Marilyn Friedman and Andy Clark (eds.). *Mind and Morals: Essays on Ethics and Cognitive Science*. (Alternative title: *Mind and Morals: Essays on Cognitive Science and Ethics*.) Cambridge, MA, MIT Press, 1996.

Mayo, Deborah G. "Learning from Error, Severe Testing, and the Growth of Theoretical Knowledge". Chapter 1 of Deborah G. Mayo and Aris Spanos (eds.), *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*. Cambridge, Cambridge University Press, 2009.

Mayo, Deborah G., and Aris Spanos (eds.). *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*. Cambridge, Cambridge University Press, 2009.

Mayo-Wilson, Conor. "Reliability of Testimonial Norms in Scientific Communities". *Synthese*, volume 191, number 1, 2014, pages 55-78.
http://dx.doi.org/10.1007/s11229-013-0320-2

McAdam, Doug, Sidney Tarrow and Charles Tilly. *Dynamics of Contention*. Cambridge, Cambridge University Press, 2001.

McCairns, R. J. Scott, and Louis Bernatchez. "Plasticity and Heritability of Morphological Variation Within and Between Parapatric Stickleback Demes". *Journal of Evolutionary Biology*, volume 25, number 6, 2012, pages 1097-1112.
http://dx.doi.org/10.1111/j.1420-9101.2012.02496.x

McCloskey, Deirdre N. *The Rhetoric of Economics*, second edition. Madison, WI, University of Wisconsin Press, 1998.

McCook, Alison. "Is Peer Review Broken?" *The Scientist*, online edition, volume 20, number 2, 2006.
http://www.the-scientist.com/?articles.view/articleNo/23672/title/Is-Peer-Review-Broken-/

McCullagh, C. Behan. "Bias in Historical Description, Interpretation, and Explanation". *History and Theory*, volume 39, number 1, 2000, pages 39-66.
http://dx.doi.org/10.1111/0018-2656.00112

McCullagh, C. Behan. "Colligation". Chapter 12 of Aviezer Tucker (ed.), *A Companion to the Philosophy of History and Historiography*. Oxford, Blackwell, 2009.

McCullagh, C. Behan. *The Truth of History*. London, Routledge, 1998.

McDowell, John. *Mind and World*, with a new introduction by the author. Cambridge, MA, Harvard University Press, 1996.

McMullin, Ernan. “The Virtues of a Good Theory”. Chapter 53 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (chapter 47 of the first edition, 2008).

McMyler, Benjamin. *Testimony, Trust, and Authority*. Oxford, Oxford University Press, 2011.

MediaCommons Press. A website that hosts texts on media studies and accepts comments on them.
http://mcpress.media-commons.org/

Meheus, Joke (ed.). *Inconsistency in Science*. Dordrecht, Kluwer Academic Publishers, 2002.
http://dx.doi.org/10.1007/978-94-017-0085-6

Meheus, Joke, and Diderik Batens. “A Formal Logic for Abductive Reasoning”. *Logic Journal of the IGPL*, volume 14, number 2, 2006, pages 221-236.
http://dx.doi.org/10.1093/jigpal/jzk015

Miles, Matthew B., A. Michael Huberman and Johnny Saldaña. *Qualitative Data Analysis: A Methods Sourcebook*, third edition. London, Sage, 2014.

Mill, John Stuart. *On Liberty*.

Miller, Daniel, and Christopher Tilley (eds.). *Ideology, Power and Prehistory*. Cambridge, Cambridge University Press, 1984.
http://dx.doi.org/10.1017/CBO9780511897443

Miller, Delbert C., and Neil J. Salkind. *Handbook of Research Design and Social Measurement*, sixth edition. London, Sage, 2002.

Miller, Nathaniel. *Euclid and His Twentieth Century Rivals: Diagrams in the Logic of Euclidean Geometry.* Stanford, CA, CSLI Publications, 2007.

Millstein, Roberta L. "Natural Selection and Causal Productivity". Chapter 8 of Hsiang-Ke Chao, Szu-Ting Chen and Roberta L. Millstein (eds.), *Mechanism and Causality in Biology and Economics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-2454-9_8

Mitchell, Sandra D. *Unsimple Truths: Science, Complexity, and Policy*. Chicago, IL, University of Chicago Press, 2009.

Mitchell, Sandra D., and Michael R. Dietrich. "Integration Without Unification: An Argument for Pluralism in the Biological Sciences". *American Naturalist*, volume 168, number S6, 2006, pages S73-S79.
http://dx.doi.org/10.1086/509050

Mittag, Daniel M. "Evidentialism". Chapter 16 of Sven Bernecker and Duncan Pritchard (eds.), *The Routledge Companion to Epistemology*. Abingdon, Routledge, 2011.

Mizrahi, Moti. "Why Arguments from Expert Opinion are Weak Arguments". *Informal Logic*, volume 33, number 1, 2013, pages 57-79.
http://ojs.uwindsor.ca/ojs/leddy/index.php/informal_logic/article/view/3656

Mokyr, Joel (ed.). *The British Industrial Revolution: An Economic Perspective*, second edition. Boulder, CO, Westview Press, 1999.

Monroe, Burt L., Michael P. Colaresi and Kevin M. Quinn. "Fightin' Words: Lexical Feature Selection and Evaluation for Identifying the Content of Political Conflict". *Political Analysis*, volume 16, number 4, 2008, pages 372-403.
http://dx.doi.org/10.1093/pan/mpn018

Moran-Ellis, Jo, Victoria D. Alexander, Ann Cronin, Mary Dickinson, Jane Fielding, Judith Sleney and Hilary Thomas. "Triangulation and Integration: Processes, Claims and Implications". *Qualitative Research*, volume 6, number 1, 2006, pages 45-59.
http://dx.doi.org/10.1177/1468794106058870

Morgan, Glenn, John L. Campbell, Colin Crouch, Ove Kaj Pedersen and Richard Whitley (eds.). *The Oxford Handbook of Comparative Institutional Analysis*. Oxford, Oxford University Press, 2010.
http://dx.doi.org/10.1093/oxfordhb/9780199233762.001.0001

Morgan, Gregory J. "Achinstein and Whewell on Theoretical Coherence". Chapter 12 of Gregory J. Morgan (ed.), *Philosophy of Science Matters: The Philosophy of Peter Achinstein*. Oxford, Oxford University Press, 2011.

Morgan, Gregory J. (ed.). *Philosophy of Science Matters: The Philosophy of Peter Achinstein*. Oxford, Oxford University Press, 2011.

Morgan, Mary S. "Models, Stories and the Economic World". *Journal of Economic Methodology*, volume 8, number 3, 2001, pages 361-384.
http://dx.doi.org/10.1080/13501780110078972

Morgan, Mary S. "Nature's Experiments and Natural Experiments in the Social Sciences". *Philosophy of the Social Sciences*, volume 43, number 3, 2013, pages 341-357. http://dx.doi.org/10.1177/0048393113489100

Morgan, Mary S., and Margaret Morrison (eds.). *Models as Mediators: Perspectives on Natural and Social Science*. Cambridge, Cambridge University Press, 1999.

Morgan, Stephen L. (ed.). *Handbook of Causal Analysis for Social Research*. Dordrecht, Springer, 2013. http://dx.doi.org/10.1007/978-94-007-6094-3

Morley, Neville. *Writing Ancient History*. London, Duckworth, 1999.

Morrison, Margaret. "Unification in Physics". Chapter 11 of Robert Batterman (ed.), *The Oxford Handbook of Philosophy of Physics*. Oxford, Oxford University Press, 2013.
http://dx.doi.org/10.1093/oxfordhb/9780195392043.013.0012

Morrison, Margaret. *Unifying Scientific Theories: Physical Concepts and Mathematical Structures*. Cambridge, Cambridge University Press, 2000.

Morrison, Margaret, and Mary S. Morgan. "Models as Mediating Instruments". Chapter 2 of Mary S. Morgan and Margaret Morrison (eds.), *Models as Mediators: Perspectives on Natural and Social Science*. Cambridge, Cambridge University Press, 1999.

Morton, Adam. "Acting to Know: A Virtue of Experimentation". Chapter 12 of Abrol Fairweather (ed.), *Virtue Epistemology Naturalized: Bridges Between Virtue Epistemology and Philosophy of Science*. Dordrecht, Springer, 2014. http://dx.doi.org/10.1007/978-3-319-04672-3_12

Morton, Rebecca B. *Methods and Models: A Guide to the Empirical Analysis of Formal Models in Political Science*. Cambridge, Cambridge University Press, 1999. http://dx.doi.org/10.1017/CBO9780511612473

Moser, Paul K. (ed.). *The Oxford Handbook of Epistemology*. Oxford, Oxford University Press, 2002. http://dx.doi.org/10.1093/0195130057.001.0001

Moses, Jonathon W., and Torbjørn L. Knutsen. *Ways of Knowing: Competing Methodologies in Social and Political Research*, second edition. Basingstoke, Palgrave Macmillan, 2012.

Mouffe, Chantal. *The Democratic Paradox*. London, Verso, 2000.

Moulines, C. Ulises. "The Ways of Holism". *Noûs*, volume 20, number 3, 1986, pages 313-330. http://dx.doi.org/10.2307/2215300

Mukherji, Shankar, and Alexander van Oudenaarden. "Synthetic Biology: Understanding Biological Design from Synthetic Circuits". *Nature Reviews Genetics*, volume 10, number 12, 2009, pages 859-871. http://dx.doi.org/10.1038/nrg2697

Munro, John. *The Monetary Origins of the 'Price Revolution': South German Silver Mining, Merchant-Banking, and Venetian Commerce, 1470-1540*. Toronto, University of Toronto Department of Economics and Institute for Policy Analysis, Working Paper number 8, revised version, 21 March 2003.
http://repec.economics.utoronto.ca/files/UT-ECIPA-MUNRO-99-02.pdf

Munz, Peter. "The Historical Narrative". Chapter 34 of Michael Bentley (ed.), *Companion to Historiography*. London, Routledge, 1997.

Museum of London Archaeology Service. *Archaeological Site Manual*, third edition. London, Museum of London, 1994.
http://www.museumoflondon.org.uk/collections-research/laarc/useful-links-downloads/

Myrvold, Wayne C. "Epistemic Values and the Value of Learning". *Synthese*, volume 187, number 2, 2012, pages 547-568.
http://dx.doi.org/10.1007/s11229-010-9860-x

Naboj Dynamical Peer Review. A website for reviews of papers that have been placed in online repositories.
http://www.naboj.com/

Nature Protocols. Journal.
http://www.nature.com/nprot/

Nederbragt, Hubertus. "Multiple Derivability and the Reliability and Stabilization of Theories". Chapter 5 of Léna Soler, Emiliano Trizio, Thomas Nickles and William C. Wimsatt (eds.), *Characterizing the Robustness of Science: After the Practice Turn in Philosophy of Science*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-2759-5_5

Nentwich, Michael, and René König. "Academia Goes Facebook? The Potential of Social Network Sites in the Scholarly Realm". Chapter 7 of Sönke Bartling and Sascha Friesike (eds.), *Opening Science: The Evolving Guide on How the Internet is Changing Research, Collaboration and Scholarly Publishing*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-00026-8_7

Newman, Mark. "EMU and Inference: What the Explanatory Model of Scientific Understanding Ignores". *European Journal for Philosophy of Science*, volume 4, number 1, 2014, pages 55-74.
http://dx.doi.org/10.1007/s13194-013-0075-0

Nicholson, Daniel J. "The Concept of Mechanism in Biology". *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, volume 43, number 1, 2012, pages 152-163.
http://dx.doi.org/10.1016/j.shpsc.2011.05.014

Nickles, Thomas. "Heuristic Appraisal: Context of Discovery or Justification?" Chapter 10 of Jutta Schickore and Friedrich Steinle (eds.), *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*. Dordrecht, Springer, 2006.
http://dx.doi.org/10.1007/1-4020-4251-5_10

Nisbet, Robert, John Elder and Gary Miner. *Handbook of Statistical Analysis and Data Mining Applications.* Burlington, MA, Academic Press, 2009.

nLab. A wiki for work in mathematics and related disciplines from the n-categorical point of view.
http://ncatlab.org/

Noble, Denis. *The Music of Life: Biology Beyond the Genome*. Oxford, Oxford University Press, 2006.

Norton, John D. "A Material Theory of Induction". *Philosophy of Science*, volume 70, number 4, 2003, pages 647-670.
http://dx.doi.org/10.1086/378858

Norton, John D. "History of Science and the Material Theory of Induction: Einstein's Quanta, Mercury's Perihelion". *European Journal for Philosophy of Science*, volume 1, number 1, 2011, pages 3-27.
http://dx.doi.org/10.1007/s13194-010-0001-7

Norton, John D. "Must Evidence Underdetermine Theory?" Chapter 1 of Martin Carrier, Don Howard, and Janet A. Kourany (eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited.* Pittsburgh, PA, University of Pittsburgh Press, 2008.

Nosek, Brian A., et al. "Promoting an Open Research Culture". *Science*, volume 348, number 6242, 2015, pages 1422-1425.
http://dx.doi.org/10.1126/science.aab2374

Nosek, Brian A., and Daniël Lakens. “Registered Reports: A Method to Increase the Credibility of Published Results”. *Social Psychology*, volume 45, number 3, 2014, pages 137-141.
http://dx.doi.org/10.1027/1864-9335/a000192

Oakeshott, Michael. *Experience and its Modes*. Cambridge, Cambridge University Press, 1933.

Oddie, Graham. “Conditionalization, Cogency, and Cognitive Value”. *British Journal for the Philosophy of Science*, volume 48, number 4, 1997, pages 533-541.
http://dx.doi.org/10.1093/bjps/48.4.533

Odell, George H. *Lithic Analysis*. New York, Kluwer Academic Publishers, 2004.
http://dx.doi.org/10.1007/978-1-4419-9009-9

Oevermann, Ulrich. “Manifest der objektiv hermeneutischen Sozialforschung”. Chapter 9 of Jurij Fikfak, Frane Adam and Detlef Garz (eds.), *Qualitative Research: Different Perspectives, Emerging Trends*. Ljubljana, ZRC, 2004.

O’Hagan, Anthony, and Mike West (eds.). *The Oxford Handbook of Applied Bayesian Analysis*. Oxford, Oxford University Press, 2010.

O’Hear, Anthony (ed.). *Verstehen and Humane Understanding*, Royal Institute of Philosophy Supplement 41. Cambridge, Cambridge University Press, 1996.

Okasha, Samir. *Evolution and the Levels of Selection*. Oxford, Clarendon Press, 2006.

Oliver, Alex, and Timothy Smiley. *Plural Logic*. Oxford, Oxford University Press, 2013.

Olsson, Erik J. "The Impossibility of Coherence". *Erkenntnis*, volume 63, number 3, 2005, pages 387-412.
http://dx.doi.org/10.1007/s10670-005-4007-z

Open Science Collaboration. "Estimating the Reproducibility of Psychological Science". *Science*, volume 349, number 6251, 2015, pages aac4716-1 to aac4716-8.
http://dx.doi.org/10.1126/science.aac4716

OpenWetWare. A wiki for work in biology.
http://openwetware.org/

Opp, Karl-Dieter, and Christiane Gern. "Dissident Groups, Personal Networks, and Spontaneous Cooperation: The East German Revolution of 1989". *American Sociological Review*, volume 58, number 5, 1993, pages 659-680.
http://www.jstor.org/stable/2096280

Oreskes, Naomi. *The Rejection of Continental Drift: Theory and Method in American Earth Science*. Oxford, Oxford University Press, 1999.

Osgood, Josiah. *Caesar's Legacy: Civil War and the Emergence of the Roman Empire*. Cambridge, Cambridge University Press, 2006.

Ott, Joachim, and Martin Treu (eds.). *Luthers Thesenanschlag – Faktum oder Fiktion*. (Alternative title: *Faszination Thesenanschlag – Faktum oder Fiktion*.) Leipzig, Evangelische Verlagsanstalt, 2008.

Oxford e-Research Centre website.
http://www.oerc.ox.ac.uk/

Oxford e-Social Science Project website.
http://microsites.oii.ox.ac.uk/oess/

Packer, Dominic J. “Avoiding Groupthink: Whereas Weakly Identified Members Remain Silent, Strongly Identified Members Dissent About Collective Problems”. *Psychological Science*, volume 20, number 5, 2009, pages 546-548.
http://dx.doi.org/10.1111/j.1467-9280.2009.02333.x

Palermos, Spyridon Orestis. *Extending Cognition in Epistemology: Towards an Individualistic Social Epistemology.* PhD thesis, University of Edinburgh, 2012.
https://www.era.lib.ed.ac.uk/handle/1842/7972

Palermos, Spyridon Orestis, and Duncan Pritchard. “Extended Knowledge and Social Epistemology”. *Social Epistemology Review and Reply Collective*, volume 2, number 8, 2013, pages 105-120.
http://social-epistemology.com/2013/07/24/

Palmer, William G. “The Burden of Proof: J. H. Hexter and Christopher Hill”. *Journal of British Studies*, volume 19, number 1, 1979, pages 122-129.
http://dx.doi.org/10.1086/385750

Papineau, David. “Physicalism and the Human Sciences”. Chapter 4 of Chrysostomos Mantzavinos (ed.), *Philosophy of the Social Sciences: Philosophical Theory and Scientific Practice*. Cambridge, Cambridge University Press, 2009.

Parker, Wendy S. “Computer Simulation”. Chapter 13 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014. (Not in the first edition, 2008.)

Parsons, Jeffrey, and Yair Wand. “A Question of Class”. *Nature*, volume 455, number 7216, 2008, pages 1040-1041.
http://dx.doi.org/10.1038/4551040a

Pavičić, Mladen, and Norman D. Megill. “Is Quantum Logic a Logic?” Chapter 2 of Kurt Engesser, Dov M. Gabbay and Daniel Lehmann (eds.), *Handbook of Quantum Logic and Quantum Structures: Quantum Logic*. Amsterdam, Elsevier, 2009.

Paz-Fuchs, Amir. *The Social Contract Revisited: The Modern Welfare State – Overview and Critical Report*. Oxford, The Foundation for Law, Justice and Society, 2012.
http://www.fljs.org/content/social-contract-revisited-modern-welfare-state

Pearce, Mark. “The Spirit of the Sword and Spear”. *Cambridge Archaeological Journal*, volume 23, number 1, 2013, pages 55-67.
http://dx.doi.org/10.1017/S0959774313000048

Pearl, Judea. *Causality: Models, Reasoning, and Inference*, second edition. Cambridge, Cambridge University Press, 2009.

Pearson, Helen. “What is a Gene?” *Nature*, volume 441, number 7092, 2006, pages 398-401.
http://dx.doi.org/10.1038/441398a

Perman, Michael. “Eric Foner’s *Reconstruction*: A Finished Revolution”. *Reviews in American History*, volume 17, number 1, 1989, pages 73-78.
http://www.jstor.org/stable/2703129

Persson, Johannes. “Three Conceptions of Explaining How Possibly − and One Reductive Account”. Chapter 24 of Henk W. de Regt, Stephan Hartmann and Samir Okasha (eds.), *EPSA Philosophy of Science: Amsterdam 2009. The European Philosophy of Science Association Proceedings, volume 1*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-2404-4_24

Peterka, Darcy S., Hiroto Takahashi and Rafael Yuste. “Imaging Voltage in Neurons”. *Neuron*, volume 69, number 1, 2011, pages 9-21.
http://dx.doi.org/10.1016/j.neuron.2010.12.010

Pettigrew, Richard. “Epistemic Utility and Norms for Credences”. *Philosophy Compass*, volume 8, number 10, 2013, pages 897-908.
http://dx.doi.org/10.1111/phc3.12079

Pfeiffer, Thomas, Lars Bertram and John P. A. Ioannidis. “Quantifying Selective Reporting and the Proteus Phenomenon for Multiple Datasets with Similar Bias”. *PLOS ONE*, volume 6, number 3, 2011, e18362, pages 1-8.
http://dx.doi.org/10.1371/journal.pone.0018362

Pfeiffer, Thomas, and Robert Hoffmann. “Large-Scale Assessment of the Effect of Popularity on the Reliability of Research”. *PLOS ONE*, volume 4, number 6, 2009, e5996, pages 1-4.
http://dx.doi.org/10.1371/journal.pone.0005996

Pfeiffer, Thomas, David G. Rand and Anna Dreber. “Decision-Making in Research Tasks with Sequential Testing”. *PLOS ONE*, volume 4, number 2, 2009, e4607, pages 1-8.
http://dx.doi.org/10.1371/journal.pone.0004607

Philosophy of Science Association 24th Biennial Meeting, preprint volume, version of 7 November 2014. Pittsburgh, PA, University of Pittsburgh for the Philosophy of Science Association, 2014. (The papers from the meeting should be published in due course in *Philosophy of Science*, probably in two parts in December 2015 and December 2016.)
http://philsci-archive.pitt.edu/11134/

Piela, Lucjan. *Ideas of Quantum Chemistry*, second edition. Amsterdam, Elsevier, 2014.

Pigliucci, Massimo. "Historical vs. Nomological Sciences". *Skeptical Inquirer*, volume 35, number 1, 2011, pages 20-21.

Pincus, Steven. "Rethinking Revolutions: a Neo-Tocquevillian Perspective". Chapter 17 of Carles Boix and Susan C. Stokes (eds.), *The Oxford Handbook of Comparative Politics*. Oxford, Oxford University Press, 2007.
http://dx.doi.org/10.1093/oxfordhb/9780199566020.003.0017

Pingali, Kalyana C., Troy Shinbrot, Stephen V. Hammond and Fernando J. Muzzio. "An Observed Correlation Between Flow and Electrical Properties of Pharmaceutical Blends". *Powder Technology*, volume 192, number 2, 2009, pages 157-165.
http://dx.doi.org/10.1016/j.powtec.2008.12.012

Pitcher, Luke. *Writing Ancient History: An Introduction to Classical Historiography*. London, I. B. Tauris, 2009.

Pitici, Mircea (ed.). *The Best Writing on Mathematics 2010*. Princeton, NJ, Princeton University Press, 2011.

Pizzorno, Alessandro. "Rational Choice". Chapter 9 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Planat, Michel (ed.). *Noise, Oscillators and Algebraic Randomness: From Noise in Communication Systems to Number Theory. Lectures of a School Held in Chapelle des Bois, France, April 5-10, 1999*. Berlin, Springer, 2000.

Plato. *Phaedrus*.

PLOS ONE. Journal.
http://www.plosone.org/

Pocock, John G. A. "The Reconstruction of Discourse: Towards the Historiography of Political Thought". Chapter 5 of Pocock, *Political Thought and History: Essays on Theory and Method*. Cambridge, Cambridge University Press, 2009.

Pollard, A. Mark. "Measuring the Passage of Time: Achievements and Challenges in Archaeological Dating". Chapter 6 of Barry Cunliffe, Chris Gosden and Rosemary A. Joyce (eds.), *The Oxford Handbook of Archaeology*. Oxford, Oxford University Press, 2009.
http://dx.doi.org/10.1093/oxfordhb/9780199271016.013.0006

Polymath. A set of collaborative projects to solve mathematical problems.
http://polymathprojects.org/

Popper, Karl R. *The Logic of Scientific Discovery*, Routledge Classics edition. London, Routledge, 2002.

Popper, Karl R., edited by M. A. Notturno. *The Myth of the Framework: In Defence of Science and Rationality.* Abingdon, Routledge, 1994.

Popper, Karl R. *The Open Society and its Enemies*, Routledge Classics edition with a preface by Václav Havel. Abingdon, Routledge, 2011.

Portides, Demetris. "Models". Chapter 40 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (chapter 36 of the first edition, 2008).

Poteete, Amy R., Marco A. Janssen and Elinor Ostrom. *Working Together: Collective Action, the Commons, and Multiple Methods in Practice.* Princeton, NJ, Princeton University Press, 2010.

Prestwich, Michael. *Plantagenet England 1225-1360.* Oxford, Clarendon Press, 2005.

Price, Huw. "Truth as Convenient Friction". *The Journal of Philosophy*, volume 100, number 4, 2003, pages 167-190. http://www.jstor.org/stable/3655652

Price, Huw, and Richard Corry (eds.). *Causation, Physics, and the Constitution of Reality: Russell's Republic Revisited.* Oxford, Clarendon Press, 2007.

Przeworski, Adam. "Is the Science of Comparative Politics Possible?" Chapter 6 of Carles Boix and Susan C. Stokes (eds.), *The Oxford Handbook of Comparative Politics.* Oxford, Oxford University Press, 2007. http://dx.doi.org/10.1093/oxfordhb/9780199566020.003.0006

PubPeer. A website for reviews of papers that have been published or placed in online repositories.
http://pubpeer.com/

Pumfrey, Stephen, Paul Rayson and John Mariani. "Experiments in 17th Century English: Manual versus Automatic Conceptual History". *Literary and Linguistic Computing*, volume 27, number 4, 2012, pages 395-408. (In December 2014, the journal title changed to *Digital Scholarship in the Humanities*.)
http://dx.doi.org/10.1093/llc/fqs017

Puschmann, Cornelius. "(Micro)Blogging Science? Notes on Potentials and Constraints of New Forms of Scholarly Communication". Chapter 6 of Sönke Bartling and Sascha Friesike (eds.), *Opening Science: The Evolving Guide on How the Internet is Changing Research, Collaboration and Scholarly Publishing*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-00026-8_6

Quine, Willard Van Orman. "Two Dogmas of Empiricism". Chapter 2 of Quine, *From a Logical Point of View: Nine Logico-Philosophical Essays*, second edition, revised. Cambridge, MA, Harvard University Press, 1980.

Raffman, Diana. *Unruly Words: A Study of Vague Language*. Oxford, Oxford University Press, 2014.
http://dx.doi.org/10.1093/acprof:oso/9780199915101.001.0001

Ragin, Charles C. *Redesigning Social Inquiry: Fuzzy Sets and Beyond*. Chicago, IL, University of Chicago Press, 2008.

Ramey, Valerie A. "Identifying Government Spending Shocks: It's all in the Timing". *The Quarterly Journal of Economics*, volume 126, number 1, 2011, pages 1-50.
http://dx.doi.org/10.1093/qje/qjq008

Ramsey, William M. *Representation Reconsidered*. Cambridge, Cambridge University Press, 2007.

Randall, David A., Richard A. Wood, Sandrine Bony, Robert Colman, Thierry Fichefet, John Fyfe, Vladimir Kattsov, Andrew Pitman, Jagadish Shukla, Jayaraman Srinivasan, Ronald J. Stouffer, Akimasa Sumi and Karl E. Taylor. "Climate Models and Their Evaluation". Chapter 8 of Susan Solomon, Dahe Qin, Martin Manning, Zhenlin Chen, Melinda Marquis, Kristen B. Averyt, Melinda M. B. Tignor and Henry LeRoy Miller (eds.), *Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge, Cambridge University Press, 2007.

Rehmeyer, Julie. "Crowdsourcing Peer Review". *Science News*, online edition, 9 September 2010.
https://www.sciencenews.org/article/crowdsourcing-peer-review

Reiss, John O. *Not by Design: Retiring Darwin's Watchmaker*. Berkeley, CA, University of California Press, 2009.

Reiss, Julian. *Philosophy of Economics: A Contemporary Introduction*. Abingdon, Routledge, 2013.

Rescher, Nicholas. *Unknowability: An Inquiry into the Limits of Knowledge*. Lanham, MD, Lexington Books, 2009.

Retraction Watch. A website that lists and discusses retractions of papers.
http://retractionwatch.com/

Rheinberger, Hans-Jörg. *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Stanford, CA, Stanford University Press, 1997.

Rheinberger, Hans-Jörg, and Staffan Müller-Wille. "Gene Concepts". Chapter 1 of Sahotra Sarkar and Anya Plutynski (eds.), *A Companion to the Philosophy of Biology*. Oxford, Blackwell, 2008.

Richter, Melvin. *The History of Political and Social Concepts: A Critical Introduction*. Oxford, Oxford University Press, 1995.

Rickman, Hans Peter. *Wilhelm Dilthey: Pioneer of the Human Studies*. London, Paul Elek, 1979.

Riegler, Alexander, and Igor Douven. "Extending the Hegselmann-Krause Model II". Chapter 12 of Tadeusz Czarnecki, Katarzyna Kijania-Placek, Olga Poller and Jan Woleński (eds.), *The Analytical Way: Proceedings of the 6th European Congress of Analytic Philosophy*. London, College Publications, 2010.

Riegler, Alexander, and Igor Douven. "Extending the Hegselmann-Krause Model III: From Single Beliefs to Complex Belief States". *Episteme*, volume 6, number 2, 2009, pages 145-163.
http://dx.doi.org/10.3366/E1742360009000616

Rieh, Soo Young. "Judgment of Information Quality and Cognitive Authority in the Web". *Journal of the American Society for Information Science and Technology*, volume 53, number 2, 2002, pages 145-161.
http://dx.doi.org/10.1002/asi.10017

Rieseberg, Loren H., Troy E. Wood and Eric J. Baack. "The Nature of Plant Species". *Nature*, volume 440, number 7083, 2006, pages 524-527.
http://dx.doi.org/10.1038/nature04402

Rihoux, Benoît. "Case-Oriented Configurational Research: Qualitative Comparative Analysis (QCA), Fuzzy Sets, and Related Techniques". Chapter 31 of Janet M. Box-Steffensmeier, Henry E. Brady and David Collier (eds.), *The Oxford Handbook of Political Methodology*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/oxfordhb/9780199286546.003.0031

Roberts, Clayton. *The Logic of Historical Explanation*. University Park, PA, Pennsylvania State University Press, 1996.

Roberts, Geoffrey (ed.). *The History and Narrative Reader*. London, Routledge, 2001.

Roberts, John T. *The Law-Governed Universe*. Oxford, Oxford University Press, 2008.

Rolin, Kristina. "Scientific Knowledge: A Stakeholder Theory". Chapter 3 of Jeroen van Bouwel (ed.), *The Social Sciences and Democracy*. Basingstoke, Palgrave Macmillan, 2009.

Ronce, Ophélie. "Geographic Variation, Population Structure, and Migration". Section 4.3 of Jonathan B. Losos et al. (eds.), *The Princeton Guide to Evolution*. Princeton, NJ, Princeton University Press, 2014.

Rose, James D. "Diverse Perspectives on the Groupthink Theory – A Literary Review". *Emerging Leadership Journeys*, volume 4, number 1, 2011, pages 37-57.
http://www.regent.edu/acad/global/publications/elj/vol4iss1/Rose_V4I1_pp37-57.pdf

Rosenberg, Alex. “How is Biological Explanation Possible?” *British Journal for the Philosophy of Science*, volume 52, number 4, 2001, pages 735-760.
http://dx.doi.org/10.1093/bjps/52.4.735

Roskams, Steve. *Excavation*. Cambridge, Cambridge University Press, 2001.

Ross, Don. “Psychological versus Economic Models of Bounded Rationality”. *Journal of Economic Methodology*, volume 21, number 4, 2014, pages 411-427.
http://dx.doi.org/10.1080/1350178X.2014.965910

Rothbauer, Paulette M. “Triangulation”. Volume 2, pages 892-894, of Lisa M. Given (ed.), *The Sage Encyclopedia of Qualitative Research Methods*, two volumes. London, Sage, 2008.
http://dx.doi.org/10.4135/9781412963909

Rothwell, Peter M. “Commentary: External Validity of Results of Randomized Trials: Disentangling a Complex Concept”. *International Journal of Epidemiology*, volume 39, number 1, 2010, pages 94-96.
http://dx.doi.org/10.1093/ije/dyp305

Roxburgh, Stephen H., Katriona Shea and J. Bastow Wilson. “The Intermediate Disturbance Hypothesis: Patch Dynamics and Mechanisms of Species Coexistence”. *Ecology*, volume 85, number 2, 2004, pages 359-371.
http://dx.doi.org/10.1890/03-0266

Royal Society Science Policy Centre. *Science as an Open Enterprise: Final Report* (report 02/12). London, The Royal Society, 2012.
https://royalsociety.org/policy/projects/science-public-enterprise/Report/

Royal Swedish Academy of Sciences. *Development of Multiscale Models for Complex Chemical Systems: Scientific Background on the Nobel Prize in Chemistry 2013*. Stockholm, Royal Swedish Academy of Sciences, 2013.
http://www.nobelprize.org/nobel_prizes/chemistry/laureates/2013/advanced-chemistryprize2013.pdf

Rueschemeyer, Dietrich. “Can One or a Few Cases Yield Theoretical Gains?” Chapter 9 of James Mahoney and Dietrich Rueschemeyer (eds.), *Comparative Historical Analysis in the Social Sciences*. Cambridge, Cambridge University Press, 2003.

Rupert, Robert D. “*Ceteris Paribus* Laws, Component Forces, and the Nature of Special-Science Properties”. *Noûs*, volume 42, number 3, 2008, pages 349-380.
http://dx.doi.org/10.1111/j.1468-0068.2008.00685.x

Ruse, Michael (ed.). *The Oxford Handbook of Philosophy of Biology*. Oxford, Oxford University Press, 2008.

Ruxton, Graeme D., and Nick Colegrave. *Experimental Design for the Life Sciences*, third edition. Oxford, Oxford University Press, 2011.

Sabean, David Warren. *Property, Production, and Family in Neckarhausen, 1700-1870*. Cambridge, Cambridge University Press, 1990.

Sachsenmaier, Dominic. *Global Perspectives on Global History: Theories and Approaches in a Connected World*. Cambridge, Cambridge University Press, 2011.

Sandkühler, Hans Jörg (ed.). *Enzyklopädie Philosophie*, two volumes. Hamburg, Felix Meiner, 1999.

Sanford, Anthony J. (ed.). *The Nature and Limits of Human Understanding*. London, Continuum, 2003.

Sarkar, Sahotra, and Anya Plutynski (eds.). *A Companion to the Philosophy of Biology*. Oxford, Blackwell, 2008.

Scaramuzzi, Francesco. "Ten Years of Cold Fusion: An Eye-Witness Account". *Accountability in Research: Policies and Quality Assurance*, volume 8, numbers 1-2, 2000, pages 77-101.
http://dx.doi.org/10.1080/08989620008573967

Scarre, Geoffrey, and Robin Coningham (eds.). *Appropriating the Past: Philosophical Perspectives on the Practice of Archaeology*. Cambridge, Cambridge University Press, 2013.
http://dx.doi.org/10.1017/CBO9781139026932

Schickore, Jutta, and Friedrich Steinle (eds.). *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*. Dordrecht, Springer, 2006.
http://dx.doi.org/10.1007/1-4020-4251-5

Schiffer, Stephen. *The Things We Mean*. Oxford, Clarendon Press, 2003.
http://dx.doi.org/10.1093/0199257760.001.0001

Schiffer, Stephen. "Vague Properties". Chapter 6 of Richard Dietz and Sebastiano Moruzzi (eds.), *Cuts and Clouds: Vagueness, its Nature and its Logic*. Oxford, Oxford University Press, 2010.

Schimmack, Ulrich. "The Ironic Effect of Significant Results on the Credibility of Multiple-Study Articles". *Psychological Methods*, volume 17, number 4, 2012, pages 551-566.
http://dx.doi.org/10.1037/a0029487

Schmidt, Lawrence K. *Understanding Hermeneutics*. Durham, Acumen, 2006.

Scholarpedia. A peer-reviewed online encyclopedia.
http://www.scholarpedia.org/

Scholz, Oliver Robert. "Verstehen". Volume 2, pages 1698-1702, of Hans Jörg Sandkühler (ed.), *Enzyklopädie Philosophie*. Hamburg, Felix Meiner, 1999.

Schouten, Maurice, and Huib Looren de Jong (eds.). *The Matter of the Mind: Philosophical Essays on Psychology, Neuroscience, and Reduction*. Oxford, Blackwell, 2007.

Schrenk, Markus. *The Metaphysics of Ceteris Paribus Laws*. Heusenstamm, Ontos, 2007.

Schulte, Regina, and Xenia von Tippelskirch (eds.). *Reading, Interpreting and Historicizing: Letters as Historical Sources*, EUI Working Paper HEC No. 2004/2. Badia Fiesolana, European University Institute, 2004.
http://hdl.handle.net/1814/2600

Schupbach, Jonah N., and Jan Sprenger. "The Logic of Explanatory Power". *Philosophy of Science*, volume 78, number 1, 2011, pages 105-127.
http://dx.doi.org/10.1086/658111

Schutt, Russell K. *Investigating the Social World: The Process and Practice of Research*, eighth edition. London, Sage, 2015.

Schwirian, Kent P. "Models of Neighborhood Change". *Annual Review of Sociology*, volume 9, 1983, pages 83-102.
http://dx.doi.org/10.1146/annurev.so.09.080183.000503

Scientific Reports. Journal.
http://www.nature.com/srep/index.html

Screpanti, Ernesto, and Stefano Zamagni. *An Outline of the History of Economic Thought*, second edition, translated by David Field and Lynn Kirby. Oxford, Oxford University Press, 2005.

Seebohm, Thomas M. *Hermeneutics. Method and Methodology*. Dordrecht, Kluwer Academic Publishers, 2004.
http://dx.doi.org/10.1007/978-1-4020-2618-8

Seidl, Christian, Ulrich Schmidt and Peter Grösche. "The Performance of Peer Review and a Beauty Contest of Referee Processes of Economics Journals". *Estudios de Economía Aplicada*, volume 23, number 3, 2005, pages 505-551.
http://www.revista-eea.net/volumen.php?Id=57&vol=23&ref=3

SelectedPapers.net. A platform for sharing comments on papers that have been placed in online repositories.
http://docs.selectedpapers.net/

Shapiro, Stewart. "Varieties of Pluralism and Relativism for Logic". Chapter 27 of Steven D. Hales (ed.), *A Companion to Relativism*. Oxford, Blackwell, 2011.

Shapiro, Stewart. "We Hold These Truths To Be Self-Evident: But What Do We Mean By That?" *Review of Symbolic Logic*, volume 2, number 1, 2009, pages 175-207.
http://dx.doi.org/10.1017/S175502030909011X

Shaughnessy, John J., Eugene B. Zechmeister and Jeanne S. Zechmeister. *Research Methods in Psychology*, tenth edition. New York, NY, McGraw-Hill, 2014.

Shavitt, Isaiah, and Rodney J. Bartlett. *Many-Body Methods in Chemistry and Physics: MBPT and Coupled-Cluster Theory*. Cambridge, Cambridge University Press, 2009.

Sheredos, Benjamin, Daniel Burnston, Adele Abrahamsen and William Bechtel. "Why Do Biologists Use So Many Diagrams?" *Philosophy of Science*, volume 80, number 5, 2013, pages 931-944.
http://dx.doi.org/10.1086/674047

Shimer, Robert. "The Cyclical Behavior of Equilibrium Unemployment and Vacancies". *American Economic Review*, volume 95, number 1, 2005, pages 25-49.
http://dx.doi.org/10.1257/0002828053828572

Shin, Sun-Joo. *The Logical Status of Diagrams*. Cambridge, Cambridge University Press, 1994.

Shogenji, Tomoji. "The Role of Coherence of Evidence in the Non-Dynamic Model of Confirmation". *Erkenntnis*, volume 63, number 3, 2005, pages 317-333.
http://dx.doi.org/10.1007/s10670-005-4001-5

Shoham, Yoav, and Kevin Leyton-Brown. *Multiagent Systems: Algorithmic, Game-Theoretic, and Logical Foundations*. Cambridge, Cambridge University Press, 2009.

Siegel, Larry J. *Criminology: the Core*, fifth edition. Stamford, CT, Cengage Learning, 2015.

Silm, Siiri, and Rein Ahas. "The Temporal Variation of Ethnic Segregation in a City: Evidence from a Mobile Phone Use Dataset". *Social Science Research*, volume 47, 2014, pages 30-43.
http://dx.doi.org/10.1016/j.ssresearch.2014.03.011

Simonsohn, Uri, Leif D. Nelson and Joseph P. Simmons. "p-Curve and Effect Size: Correcting for Publication Bias Using Only Significant Results". *Perspectives on Psychological Science*, volume 9, number 6, 2014, pages 666-681.
http://dx.doi.org/10.1177/1745691614553988

Simpson, Stephen G. *Subsystems of Second Order Arithmetic*, second edition. Cambridge, Cambridge University Press, 2009.

Skinner, Quentin. "Meaning and Understanding in the History of Ideas". *History and Theory*, volume 8, number 1, 1969, pages 3-53.
http://www.jstor.org/stable/2504188

Skinner, Quentin (ed.). *The Return of Grand Theory in the Human Sciences*. Cambridge, Cambridge University Press, 1985.

Skinner, Quentin. *Visions of Politics: Volume 1, Regarding Method*. Cambridge, Cambridge University Press, 2002.

Skoog, Douglas A., F. James Holler and Stanley R. Crouch. *Principles of Instrumental Analysis*, sixth edition. Belmont, CA, Thomson Brooks/Cole, 2007.

Smith, George Davey, and Shah Ebrahim. "Data Dredging, Bias, or Confounding: They Can All Get You into the BMJ and the Friday Papers". *British Medical Journal*, volume 325, number 7378, 2002, pages 1437-1438.
http://dx.doi.org/10.1136/bmj.325.7378.1437

Smith, Neil. *The New Urban Frontier: Gentrification and the Revanchist City*. London, Routledge, 1996.

Smith, Nicholas J. J. *Vagueness and Degrees of Truth*. Oxford, Oxford University Press, 2008.

Smith, Peter. *An Introduction to Gödel's Theorems*, second edition. Cambridge, Cambridge University Press, 2013.

Smithson, Michael, and Jay Verkuilen. *Fuzzy Set Theory: Applications in the Social Sciences*. London, Sage, 2006.

Smolin, Lee. *The Trouble with Physics: The Rise of String Theory, The Fall of a Science, and What Comes Next*. London, Allen Lane, 2006.

Snowdon, Brian, and Howard R. Vane. *Modern Macroeconomics: Its Origins, Development and Current State*. Cheltenham, Edward Elgar, 2005.

Snyder, Laura J. "Consilience, Confirmation, and Realism". Chapter 7 of Peter Achinstein (ed.), *Scientific Evidence: Philosophical Theories and Applications*. Baltimore, MD, Johns Hopkins University Press, 2005.

Social Science Research Network. An online repository for papers.
http://ssrn.com/

Soler, Léna, Emiliano Trizio, Thomas Nickles and William C. Wimsatt (eds.). *Characterizing the Robustness of Science: After the Practice Turn in Philosophy of Science*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-94-007-2759-5

Solomon, Miriam. "*Groupthink* versus *The Wisdom of Crowds*: The Social Epistemology of Deliberation and Dissent". *The Southern Journal of Philosophy*, volume 44, number S1, 2006, pages 28-42.
http://dx.doi.org/10.1111/j.2041-6962.2006.tb00028.x

Solomon, Susan, Dahe Qin, Martin Manning, Zhenlin Chen, Melinda Marquis, Kristen B. Averyt, Melinda M. B. Tignor and Henry LeRoy Miller (eds.). *Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge, Cambridge University Press, 2007.

Sperber, Dan. *Explaining Culture: A Naturalistic Approach*. Oxford, Blackwell, 1996.

Sperber, Dan, David Premack and Ann James Premack (eds.). *Causal Cognition: A Multidisciplinary Debate*. Oxford, Clarendon Press, 1995.
http://dx.doi.org/10.1093/acprof:oso/9780198524021.001.0001

Sperber, Jonathan. *The European Revolutions, 1848-1851*, second edition. Cambridge, Cambridge University Press, 2005.

Spinney, Laura. "History as Science". *Nature*, volume 488, number 7409, 2012, pages 24-26 (correction made on 8 August 2012).
http://dx.doi.org/10.1038/488024a

Spohn, Wolfgang. "Normativity is the Key to the Difference Between the Human and the Natural Sciences". Chapter 16 of Dennis Dieks, Wenceslao J. González, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.), *Explanation, Prediction, and Confirmation*. Dordrecht, Springer, 2011.
http://dx.doi.org/10.1007/978-94-007-1180-8_16

Spohn, Wolfgang. *The Laws of Belief: Ranking Theory and its Philosophical Applications*. Oxford, Oxford University Press, 2012.

Spradley, James, and David W. McCurdy (eds.). *Conformity and Conflict: Readings in Cultural Anthropology*, fourteenth edition. London, Pearson, 2011.

St. Martin, Kevin, and Marianna Pavlovskaya. "Secondary Data". Chapter 11 of Basil Gomez and John Paul Jones III (eds.), *Research Methods in Geography: A Critical Introduction*. Oxford, Blackwell, 2010.

Stadler, Friedrich, Dennis Dieks, Wenceslao J. Gonzáles, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.). *The Present Situation in the Philosophy of Science*. Dordrecht, Springer, 2010.
http://dx.doi.org/10.1007/978-90-481-9115-4

Stanford, P. Kyle. *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*. Oxford, Oxford University Press, 2006.

Starikova, Irina. "Why do Mathematicians Need Different Ways of Presenting Mathematical Objects? The Case of Cayley Graphs". *Topoi*, volume 29, number 1, 2010, pages 41-51.
http://dx.doi.org/10.1007/s11245-009-9065-4

Steel, Daniel P. *Across the Boundaries: Extrapolation in Biology and Social Science*. Oxford, Oxford University Press, 2008.
http://dx.doi.org/10.1093/acprof:oso/9780195331448.001.0001

Steinberg, Jonathan. *Bismarck: A Life*. Oxford, Oxford University Press, 2011.

Stephenson, Niamh, and Susan Kippax. "Memory Work". Chapter 8 of Carla Willig and Wendy Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Steward, Helen. *A Metaphysics for Freedom*. Oxford, Oxford University Press, 2012.
http://dx.doi.org/10.1093/acprof:oso/9780199552054.001.0001

Stockwell, David R. B. "Improving Ecological Niche Models by Data Mining Large Environmental Datasets for Surrogate Models". *Ecological Modelling*, volume 192, numbers 1-2, 2006, pages 188-196.
http://dx.doi.org/10.1016/j.ecolmodel.2005.05.029

Stolberg, Harald O., Geoffrey Norman and Isabelle Trop. "Randomized Controlled Trials". *American Journal of Roentgenology*, volume 183, number 6, 2004, pages 1539-1544.
http://dx.doi.org/10.2214/ajr.183.6.01831539

Stone, Lawrence. "The Revival of Narrative: Reflections on a New Old History". *Past and Present*, volume 85, number 1, 1979, pages 3-24.
http://dx.doi.org/10.1093/past/85.1.3

Stout, Lynn A. "Taking Conscience Seriously". Chapter 8 of Paul J. Zak (ed.), *Moral Markets: The Critical Role of Values in the Economy*. Princeton, NJ, Princeton University Press, 2008.

Straßer, Christian. *Adaptive Logics for Defeasible Reasoning: Applications in Argumentation, Normative Reasoning and Default Reasoning*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-00792-2

Straßer, Christian, Dunja Šešelja and Jan Willem Wieland. "Withstanding Tensions: Scientific Disagreement and Epistemic Tolerance". Chapter 6 of Emiliano Ippoliti (ed.), *Heuristic Reasoning*. Dordrecht, Springer, 2015.
http://dx.doi.org/10.1007/978-3-319-09159-4_6

Styer, Daniel F., Miranda S. Balkin, Kathryn M. Becker, Matthew R. Burns, Christopher E. Dudley, Scott T. Forth, Jeremy S. Gaumer, Mark A. Kramer, David C. Oertel, Leonard H. Park, Marie T. Rinkoski, Clait T. Smith and Timothy D. Wotherspoon. "Nine Formulations of Quantum Mechanics". *American Journal of Physics*, volume 70, number 3, 2002, pages 288-297.
http://dx.doi.org/10.1119/1.1445404

Suárez, Mauricio, Mauro Dorato and Miklós Rédei (eds.). *EPSA Epistemology and Methodology of Science: Launch of the European Philosophy of Science Association, Volume 1*. Dordrecht, Springer, 2010.
http://dx.doi.org/10.1007/978-90-481-3263-8

Suárez, Mauricio, Mauro Dorato and Miklós Rédei (eds.). *EPSA Philosophical Issues in the Sciences: Launch of the European Philosophy of Science Association, Volume 2*. Dordrecht, Springer, 2010.
http://dx.doi.org/10.1007/978-90-481-3252-2

Sue, Christian, Jean Robert Grasso, Franz Lahaie and David Amitrano. "Mechanical Behavior of Western Alpine Structures Inferred From Statistical Analysis of Seismicity". *Geophysical Research Letters*, volume 29, number 8, 2002, pages 65-1 to 65-4.
http://dx.doi.org/10.1029/2001GL014050

Suetonius. *Divus Iulius*. In Suetonius, *Lives of the Caesars, volume 1*, with a translation by J. C. Rolfe and an introduction by K. R. Bradley, revised edition. Loeb Classical Library. Cambridge, MA, Harvard University Press, 1998.

Sugden, Robert. "Credible Worlds, Capacities and Mechanisms". *Erkenntnis*, volume 70, number 1, 2009, pages 3-27. http://dx.doi.org/10.1007/s10670-008-9134-x

Sugden, Robert. "How Fictional Accounts Can Explain". *Journal of Economic Methodology*, volume 20, number 3, 2013, pages 237-243.
http://dx.doi.org/10.1080/1350178X.2013.828872

Sullivan, Jacqueline A. "The Multiplicity of Experimental Protocols: A Challenge to Reductionist and Non-Reductionist Models of the Unity of Neuroscience". *Synthese*, volume 167, number 3, 2009, pages 511-539.
http://dx.doi.org/10.1007/s11229-008-9389-4

Susskind, Leonard, and Art Friedman. *Quantum Mechanics: The Theoretical Minimum*. London, Allen Lane, 2014.

Symbolic Interaction. Journal of the Society for the Study of Symbolic Interaction. eISSN 1533-8665.
http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1533-8665

Tappenden, Jamie. "Mathematical Concepts and Definitions". Chapter 9 of Paolo Mancosu (ed.), *The Philosophy of Mathematical Practice*. Oxford, Oxford University Press, 2008.

Tappenden, Jamie. “Mathematical Concepts: Fruitfulness and Naturalness”. Chapter 10 of Paolo Mancosu (ed.), *The Philosophy of Mathematical Practice*. Oxford, Oxford University Press, 2008.

Taylor, Charles. “Interpretation and the Sciences of Man”. Chapter 1 of Taylor, *Philosophy and the Human Sciences: Philosophical Papers 2*. Cambridge, Cambridge University Press, 1985.

Taylor, Charles. “Self-Interpreting Animals”. Chapter 2 of Taylor, *Human Agency and Language: Philosophical Papers 1*. Cambridge, Cambridge University Press, 1985.

Taylor, Charles. “Understanding and Ethnocentricity”. Chapter 4 of Taylor, *Philosophy and the Human Sciences: Philosophical Papers 2*. Cambridge, Cambridge University Press, 1985.

Temin, Peter. “Price Behavior in Ancient Babylon”. *Explorations in Economic History*, volume 39, number 1, 2002, pages 46-60.
http://dx.doi.org/10.1006/exeh.2001.0774

Temin, Peter. “Two Views of the British Industrial Revolution”. *The Journal of Economic History*, volume 57, number 1, 1997, pages 63-82.
http://dx.doi.org/10.1017/S0022050700017927

Thagard, Paul. “Cognitive Science” (a). Chapter 16 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Thagard, Paul. “Cognitive Science” (b). Chapter 56 of Martin Curd and Stathis Psillos (eds.), *The Routledge Companion to Philosophy of Science*, second edition. Abingdon, Routledge, 2014 (chapter 50 of the first edition, 2008).

Thagard, Paul. “How to Collaborate: Procedural Knowledge in the Cooperative Development of Science”. *The Southern Journal of Philosophy*, volume 44, number S1, 2006, pages 177-196.
http://dx.doi.org/10.1111/j.2041-6962.2006.tb00038.x

Thagard, Paul, in collaboration with Scott Findlay, Abninder Litt, Daniel Saunders, Terrence C. Stewart and Jing Zhu. *The Cognitive Science of Science: Explanation, Discovery, and Conceptual Change*. Cambridge, MA, MIT Press, 2012.

Theiner, Georg, Colin Allen and Robert L. Goldstone. “Recognizing Group Cognition”. *Cognitive Systems Research*, volume 11, number 4, 2010, pages 378-395.
http://dx.doi.org/10.1016/j.cogsys.2010.07.002

Thies, Cameron G. “A Pragmatic Guide to Qualitative Historical Analysis in the Study of International Relations”. *International Studies Perspectives*, volume 3, number 4, 2002, pages 351-372.
http://dx.doi.org/10.1111/1528-3577.t01-1-00099

Tilley, Christopher. “Ideology and the Legitimation of Power in the Middle Neolithic of Southern Sweden”. Chapter 8 of Daniel Miller and Christopher Tilley (eds.), *Ideology, Power and Prehistory*. Cambridge, Cambridge University Press, 1984.
http://dx.doi.org/10.1017/CBO9780511897443.009

Tsai, Wan-Chen, and Wei-Ping Hu. "Theoretical Analysis on the Kinetic Isotope Effects of Bimolecular Nucleophilic Substitution (S_N2) Reactions and Their Temperature Dependence". *Molecules*, volume 18, number 4, 2013, pages 4816-4843.
http://dx.doi.org/10.3390/molecules18044816

Tucker, Aviezer (ed.). *A Companion to the Philosophy of History and Historiography*. Oxford, Blackwell, 2009.

Tufféry, Stéphane. *Data Mining and Statistics for Decision Making*, translated by Rod Riesco. Chichester, Wiley, 2011.

Tulodziecki, Dana. "Epistemic Virtues and the Success of Science". Chapter 15 of Abrol Fairweather (ed.), *Virtue Epistemology Naturalized: Bridges Between Virtue Epistemology and Philosophy of Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-04672-3_15

Tuomela, Raimo. "Holistic Social Causation and Explanation". Chapter 21 of Dennis Dieks, Wenceslao J. González, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.), *Explanation, Prediction, and Confirmation*. Dordrecht, Springer, 2011.
http://dx.doi.org/10.1007/978-94-007-1180-8_21

Turchin, Peter. "Arise 'Cliodynamics'". *Nature*, volume 454, number 7200, 2008, pages 34-35.
http://dx.doi.org/10.1038/454034a

Turner, Stephen P., and Mark W. Risjord (eds.). *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Tyler, Tom R. *Why People Obey the Law*, with a new afterword by the author. Princeton, NJ, Princeton University Press, 2006.

Udehn, Lars. *Methodological Individualism: Background, History and Meaning*. London, Routledge, 2001.

Univalent Foundations Program. *Homotopy Type Theory: Univalent Foundations of Mathematics*. Princeton, NJ, Institute for Advanced Study, 2013.
http://homotopytypetheory.org/book/

University of Auckland Centre for eResearch website.
http://www.eresearch.auckland.ac.nz/en/centre-for-eresearch.html

Urbas, Matej, Mateja Jamnik, Gem Stapleton and Jean Flower. "Speedith: A Diagrammatic Reasoner for Spider Diagrams". Chapter 19 of Philip Cox, Beryl Plimmer and Peter Rodgers (eds.), *Diagrammatic Representation and Inference: 7th International Conference, Diagrams 2012*. Dordrecht, Springer, 2012.
http://dx.doi.org/10.1007/978-3-642-31223-6_19

Vaesen, Krist, and Wybo Houkes. "Modelling the Truth of Scientific Beliefs with Cultural Evolutionary Theory". *Synthese*, volume 191, number 1, 2014, pages 109-125.
http://dx.doi.org/10.1007/s11229-013-0257-5

Van Bouwel, Jeroen. "The Problem With(out) Consensus: The Scientific Consensus, Deliberative Democracy and Agonistic Pluralism". Chapter 6 of Jeroen van Bouwel (ed.), *The Social Sciences and Democracy*. Basingstoke, Palgrave Macmillan, 2009.

Van Bouwel, Jeroen (ed.). *The Social Sciences and Democracy*. Basingstoke, Palgrave Macmillan, 2009.

Van Bouwel, Jeroen, and Erik Weber. "A Pragmatist Defense of Non-Relativistic Explanatory Pluralism in History and Social Science". *History and Theory*, volume 47, number 2, 2008, pages 168-182.
http://dx.doi.org/10.1111/j.1468-2303.2008.00445.x

Van de Mieroop, Marc. *A History of Ancient Egypt*. Chichester, Wiley, 2011.

Van der Lee, Robin, Benjamin Lang, Kai Kruse, Jörg Gsponer, Natalia Sánchez de Groot, Martijn A. Huynen, Andreas Matouschek, Monika Fuxreiter and M. Madan Babu. "Intrinsically Disordered Segments Affect Protein Half-Life in the Cell and during Evolution". *Cell Reports* volume 8, number 6, 2014, pages 1832-1844.
http://dx.doi.org/10.1016/j.celrep.2014.07.055

Van Harskamp, Anton (ed.). *Conflicts in Social Science*. London, Routledge, 1996.

Vandenberghe, Vincent, and Fábio D. Waltenberg. *Ageing Workforce, Productivity and Labour Costs of Belgian Firms*. Louvain, Institut de Recherces Économique et Sociales de l'Université catholique de Louvain, Discussion Paper number 2010-3, 2010.
http://ideas.repec.org/p/ctl/louvir/2010003.html

Vekemans, Xavier, Céline Poux, Pauline M. Goubet and Vincent Castric. "The Evolution of Selfing from Outcrossing Ancestors in Brassicaceae: What Have We Learned from Variation at the S-Locus?" *Journal of Evolutionary Biology*, volume 27, number 7, 2014, pages 1372-1385.
http://dx.doi.org/10.1111/jeb.12372

Vianello, Andrea. "Can Archaeology's 'Ritualistic and Symbolic Artefacts' Be Interpreted Semiotically?" Chapter 8 of Pilar Couto-Cantero, Gonzalo Enríquez Veloso, Alberta Passeri and José María Paz Gago (eds.), *Culture of Communication – Communication of Culture: Proceedings of the 10th World Congress of the International Association for Semiotic Studies*, online publication. A Coruña, Universidade da Coruña, 2012.
http://hdl.handle.net/2183/13396

Wallace, Brendan. *Getting Darwin Wrong: Why Evolutionary Psychology Won't Work*. Exeter, Imprint Academic, 2010.

Wallerstein, Immanuel. *World-Systems Analysis: An Introduction*. Durham, NC, Duke University Press, 2004.

Walsh, Denis. "Teleology". Chapter 5 of Michael Ruse (ed.), *The Oxford Handbook of Philosophy of Biology*. Oxford, Oxford University Press, 2008.

Wang, Rong-Lin. "Is Natural Selection a Population-Level Causal Process?" Chapter 9 of Hsiang-Ke Chao, Szu-Ting Chen and Roberta L. Millstein (eds.), *Mechanism and Causality in Biology and Economics*. Dordrecht, Springer, 2013.
http://dx.doi.org/10.1007/978-94-007-2454-9_9

Watson, Adam, and Iain Cameron. *Cool Britannia: Snowier Times in 1580-1930 than Since*. Rothersthorpe, Paragon, 2010.

Web of Science. A set of citation indices.
http://wokinfo.com/

Weber, Marcel. “Life in a Physical World: The Place of the Life Sciences”. Chapter 12 of Friedrich Stadler, Dennis Dieks, Wenceslao J. Gonzáles, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.), *The Present Situation in the Philosophy of Science*. Dordrecht, Springer, 2010.
http://dx.doi.org/10.1007/978-90-481-9115-4_12

Weber, Max. “Die „Objektivität“ sozialwissenschaftlicher und sozialpolitischer Erkenntnis”. Chapter 2 of Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*. Tübingen, J. C. B. Mohr (Paul Siebeck), 1922.

Weber, Max. “‘Objectivity’ in Social Science and Social Policy”. Chapter 2 of Weber, *On the Methodology of the Social Sciences*, translated and edited by Edward A. Shils and Henry A. Finch. Glencoe, IL, Free Press, 1949.

Weberman, David. “Gadamer’s Hermeneutics, Non-Intentionalism and the Underdeterminedness of Aesthetic Properties”. *O que nos faz Pensar*, number 18, 2004, pages 255-274.
http://www.oquenosfazpensar.com/web/index.php/numero/proxima/19

Weil, David N. *Economic Growth*, third edition. Harlow, Pearson Education, 2013.

Weingart, Peter. “A Short History of Knowledge Formations”. Chapter 1 of Robert Frodeman, Julie Thompson Klein and Karl Mitcham (eds.), *The Oxford Handbook of Interdisciplinarity*. Oxford, Oxford University Press, 2010.

Weingart, Peter. “How Robust is ‘Socially Robust Knowledge’?” Chapter 6 of Martin Carrier, Don Howard and Janet A. Kourany (eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*. Pittsburgh, PA, University of Pittsburgh Press, 2008.

Weisberg, Michael. “Who is a Modeler?” *British Journal for the Philosophy of Science*, volume 58, number 2, 2007, pages 207-233.
http://dx.doi.org/10.1093/bjps/axm011

Weisberg, Michael, and Ryan Muldoon. “Epistemic Landscapes and the Division of Cognitive Labor”. *Philosophy of Science*, volume 76, number 2, 2009, pages 225-252.
http://dx.doi.org/10.1086/644786

Wendt, Alexander E. “The Agent-Structure Problem in International Relations Theory”. *International Organization*, volume 41, number 3, 1987, pages 335-370.
http://dx.doi.org/10.1017/S002081830002751X

Werndl, Charlotte. “The Formulation and Justification of Mathematical Definitions Illustrated By Deterministic Chaos”. Chapter 26 of Mauricio Suárez, Mauro Dorato and Miklós Rédei (eds.), *EPSA Philosophical Issues in the Sciences: Launch of the European Philosophy of Science Association, Volume 2*. Dordrecht, Springer, 2010.
http://dx.doi.org/10.1007/978-90-481-3252-2_26

Wheeldon, Johannes, and Mauri K. Åhlberg. *Visualizing Social Science Research: Maps, Methods, and Meaning*. London, Sage, 2012.

Wheeler, Gregory, and Richard Scheines. “Causation, Association and Confirmation”. Chapter 3 of Dennis Dieks, Wenceslao J. González, Stephan Hartmann, Thomas Uebel and Marcel Weber (eds.), *Explanation, Prediction, and Confirmation*. Dordrecht, Springer, 2011.
http://dx.doi.org/10.1007/978-94-007-1180-8_3

White, Halbert. “A Reality Check for Data Snooping”. *Econometrica*, volume 68, number 5, 2000, pages 1097-1126.
http://dx.doi.org/10.1111/1468-0262.00152

Wickham, Chris (ed.). *Marxist History-Writing for the Twenty-First Century*. Oxford, Oxford University Press for The British Academy, 2007.
http://dx.doi.org/10.5871/bacad/9780197264034.001.0001

Wiggins, Sally, and Jonathan Potter. "Discursive Psychology". Chapter 5 of Carla Willig and Wendy Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Wigner, Eugene P. "The Unreasonable Effectiveness of Mathematics in the Natural Sciences". *Communications on Pure and Applied Mathematics*, volume 13, number 1, 1960, pages 1-14.
http://dx.doi.org/10.1002/cpa.3160130102

Wijesinghe, Hettithanthrige S., and Nicolas G. Hadjiconstantinou. "Discussion of Hybrid Atomistic-Continuum Methods for Multiscale Hydrodynamics". *International Journal for Multiscale Computational Engineering*, volume 2, number 2, 2004, pages 189-202.
http://dx.doi.org/10.1615/IntJMultCompEng.v2.i2.20

Wiklund, Maria, Ann Öhman, Carita Bengs and Eva-Britt Malmgren-Olsson. "Living Close to the Edge: Embodied Dimensions of Distress During Emerging Adulthood". *SAGE Open*, volume 4, number 2, 2014, pages 1-17.
http://dx.doi.org/10.1177/2158244014537083

Wikström, Per-Olaf H. "Why Crime Happens: A Situational Action Theory". Chapter 3 of Gianluca Manzo (ed.), *Analytical Sociology: Actions and Networks*. Chichester, Wiley, 2014.

Wilhite, Allen W., and Eric A. Fong. “Coercive Citation in Academic Publishing”. *Science*, volume 335, number 6068, 2012, pages 542-543.
http://dx.doi.org/10.1126/science.1212540

Wilkinson, Sue, and Celia Kitzinger. “Conversation Analysis”. Chapter 4 of Carla Willig and Wendy Stainton-Rogers (eds.), *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Williams, Bernard. *Ethics and the Limits of Philosophy*, with a commentary on the text by A. W. Moore. Abingdon, Routledge, 2006.

Williams, Bernard. “Making Sense of Humanity”. Chapter 7 of Williams, *Making Sense of Humanity and Other Philosophical Papers*. Cambridge, Cambridge University Press, 1995.

Williams, Bernard. “Philosophy as a Humanistic Discipline”. Chapter 16 of Williams, *Philosophy as a Humanistic Discipline*, selected, edited, and with an introduction by A. W. Moore. Princeton, NJ, Princeton University Press, 2006.

Williams, Bernard. *Shame and Necessity*. Berkeley, CA, University of California Press, 1993.

Williams, Bernard. *Truth and Truthfulness: An Essay in Genealogy*. Princeton, NJ, Princeton University Press, 2002.

Williamson, Timothy. *Knowledge and its Limits*. Oxford, Oxford University Press, 2000.

Williamson, Timothy. *The Philosophy of Philosophy*. Oxford, Blackwell, 2007.

Williamson, Timothy. *Vagueness*. London, Routledge, 1994.

Willig, Carla, and Wendy Stainton-Rogers (eds.). *The Sage Handbook of Qualitative Research in Psychology*. London, Sage, 2008.

Wimsatt, William C. *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*. Cambridge, MA, Harvard University Press, 2007.

Windelband, Wilhelm. "Geschichte und Naturwissenschaft (Straßburger Rektoratsrede, 1894)". Chapter 5 of Windelband, *Präludien: Aufsätze und Reden zur Philosophie und ihrer Geschichte*, 5., erweiterte Auflage, Band 2. Tübingen, J. C. B. Mohr (Paul Siebeck), 1915.
http://digi.ub.uni-heidelberg.de/diglit/windelband1915bd2

Windelband, Wilhelm. "History and Natural Science", translated by James T. Lamiell. *Theory and Psychology*, volume 8, number 1, 1998, pages 5-22.
http://dx.doi.org/10.1177/0959354398081001

Winther, Rasmus Grønfeldt. "Character Analysis in Cladistics: Abstraction, Reification, and the Search for Objectivity". *Acta Biotheoretica*, volume 57, numbers 1-2, 2009, pages 129-162.
http://dx.doi.org/10.1007/s10441-008-9064-7

Winther, Rasmus Grønfeldt. "Fisherian and Wrightian Perspectives in Evolutionary Genetics and Model-Mediated Imposition of Theoretical Assumptions". *Journal of Theoretical Biology*, volume 240, number 2, 2006, pages 218-232.
http://dx.doi.org/10.1016/j.jtbi.2005.09.010

Winther, Rasmus Grønfeldt. "Parts and Theories in Compositional Biology". *Biology and Philosophy*, volume 21, number 4, 2006, pages 471-499.
http://dx.doi.org/10.1007/s10539-005-9002-x

Woodward, James. "Causal Models in the Social Sciences". Chapter 4 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Woodward, James. "Causation with a Human Face". Chapter 4 of Huw Price and Richard Corry (eds.), *Causation, Physics, and the Constitution of Reality: Russell's Republic Revisited*. Oxford, Clarendon Press, 2007.

Woodward, James. *Making Things Happen: A Theory of Causal Explanation*. Oxford, Oxford University Press, 2003.

Woody, Andrea I., Robin Findlay Hendry and Paul Needham (eds.). *Philosophy of Chemistry: Handbook of the Philosophy of Science, volume 6*. Amsterdam, North-Holland, 2012.
http://www.sciencedirect.com/science/book/9780444516756

Worrall, John, and Deborah G. Mayo. "Theory Confirmation and Novel Evidence". Chapter 4 of Deborah G. Mayo and Aris Spanos (eds.), *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*. Cambridge, Cambridge University Press, 2009.

Wray, K. Brad. "Evaluating Scientists: Examining the Effects of Sexism and Nepotism". Chapter 4 of Harold Kincaid, John Dupré and Alison Wylie (eds.), *Value-Free Science? Ideals and Illusions*. Oxford, Oxford University Press, 2007.

Wray, K. Brad. “Scientific Authorship in the Age of Collaborative Research”. *Studies in History and Philosophy of Science Part A*, volume 37, number 3, 2006, pages 505-514.
http://dx.doi.org/10.1016/j.shpsa.2005.07.011

Wray, K. Brad. “The Epistemic Significance of Collaborative Research”. *Philosophy of Science*, volume 69, number 1, 2002, pages 150-168.
http://dx.doi.org/10.1086/338946

Wu, C. F. Jeff, and Michael S. Hamada. *Experiments: Planning, Analysis, and Optimization*, second edition. Hoboken, NJ, Wiley, 2009.

Wuchty, Stefan, Benjamin F. Jones and Brian Uzzi. “The Increasing Dominance of Teams in Production of Knowledge”. *Science*, volume 316, number 5827, 2007, pages 1036-1039.
http://dx.doi.org/10.1126/science.1136099.
Supporting online material at
http://www.sciencemag.org/content/316/5827/1036/suppl/DC1

Wylie, Alison. “Philosophy of Archaeology; Philosophy in Archaeology”. Chapter 14 of Stephen P. Turner and Mark W. Risjord (eds.), *Philosophy of Anthropology and Sociology: Handbook of the Philosophy of Science, volume 15*. Amsterdam, North-Holland, 2007.

Wylie, Alison. *Thinking from Things: Essays in the Philosophy of Archaeology*. Berkeley, CA, University of California Press, 2002.

Wylie, Alison, and Lynn Hankinson Nelson. "Coming to Terms with the Values of Science: Insights from Feminist Science Studies Scholarship". Chapter 3 of Harold Kincaid, John Dupré and Alison Wylie (eds.), *Value-Free Science? Ideals and Illusions*. Oxford, Oxford University Press, 2007.

Yin, Robert K. *Applications of Case Study Research*, third edition. London, Sage, 2012.

Yin, Robert K. *Case Study Research: Design and Methods*, fifth edition. London, Sage, 2014.

Yin, Robert K. *Qualitative Research from Start to Finish*. New York, NY, Guilford Press, 2011.

Ylikoski, Petri. "Micro, Macro, and Mechanisms". Chapter 2 of Harold Kincaid (ed.), *The Oxford Handbook of Philosophy of Social Science*. Oxford, Oxford University Press, 2012.
http://dx.doi.org/10.1093/oxfordhb/9780195392753.013.0002

Yu, Chen, Daniel Yurovsky and Tian(Linger) Xu. "Visual Data Mining: An Exploratory Approach to Analyzing Temporal Patterns of Eye Movements". *Infancy*, volume 17, number 1, 2012, pages 33-60.
http://dx.doi.org/10.1111/j.1532-7078.2011.00095.x

Zahle, Julie, and Finn Collin (eds.). *Rethinking the Individualism/Holism Debate: Essays in the Philosophy of Social Science*. Dordrecht, Springer, 2014.
http://dx.doi.org/10.1007/978-3-319-05344-8

Zak, Paul J. (ed.). *Moral Markets: The Critical Role of Values in the Economy*. Princeton, NJ, Princeton University Press, 2008.

Zodrow, George R. "Capital Mobility and Capital Tax Competition". *National Tax Journal*, volume 63, number 4, 2010, pages 865-901.
http://econpapers.repec.org/repec:ntj:journl:v:63:y:2010:i:4:p:865-901

Zollman, Kevin J. S. "Social Structure and the Effects of Conformity". *Synthese*, volume 172, number 3, 2010, pages 317-340.
http://dx.doi.org/10.1007/s11229-008-9393-8

Zollman, Kevin J. S. "The Epistemic Benefit of Transient Diversity". *Erkenntnis*, volume 72, number 1, 2010, pages 17-35.
http://dx.doi.org/10.1007/s10670-009-9194-6